A

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CHEMISTRY,

AND

THE ARTS.

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Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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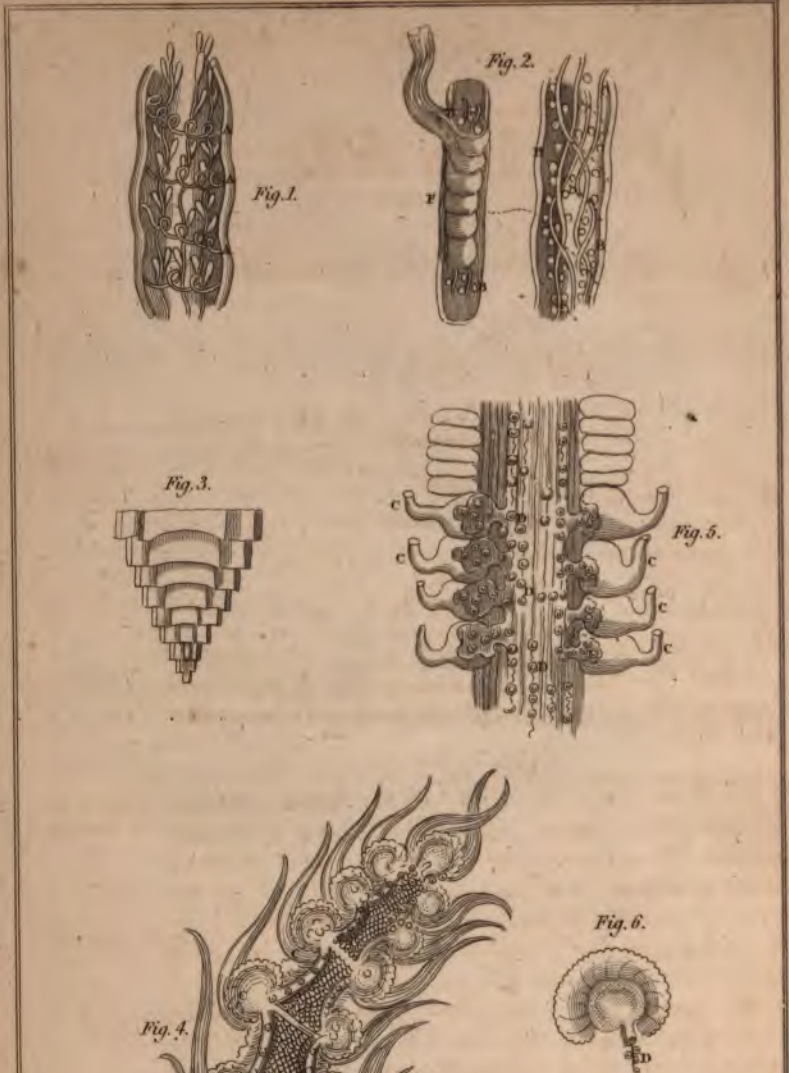

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AND

THE ARTS.

MAY, 1813.

ARTICLE I.

Method of constructing an Iron Bridge, with one arch of extensive span. Proposed to be carried over the Menai between Anglesea and Caernarvonshire. By THOMAS TELFORD, *Esq. Civil Engineer*.*

THE other design, plate I. is for the narrower streight called Ynys-y-Moch. Here the situation is particularly favourable for constructing a Bridge of one arch, and making that 500 feet span, leaves the navigation as free as at present. In this I have made the height 100 feet in the clear at high-water spring-tides, and I propose this bridge to be 40 feet in breadth :—estimating from drawings, as already described, I find the expense to be 127,331*l.* or 31,367*l.* less than the former.† From leaving the whole channel unimpeded, it is certainly the most perfect scheme of passing the Menai, and it would, in my opinion, be attended with the least inconvenience and risk in the execution. Bridge over the Menai.

* From his Report to the Lords of the Treasury, April 22, 1811.

† Two designs are given in the Report; one for the bridge here described, of a single arch, at Ynys-y-Moch, about 800 yards to the left, or south-westward, of Bagnor Ferry, and another of five arches at about twice that distance upon the Swilley rocks, estimated at 158,698*l.*

New method of centering without supports beneath.

In order to render this evident, I have made a drawing to shew in what manner the centering or frame for an arch of this magnitude may be constructed; hitherto the centering has been made by placing supports and working from below; but in the case of the Menai, from the nature of the bottom of the channel, the depth at low water, and the great rise and rapidity of the tides, this would be very difficult, if not impracticable. I therefore propose changing the mode, and working entirely from above; that is to say, instead of supporting, I mean to suspend the centering. By inspecting the drawing, the general principle of this will be readily conceived.

The abutments.

I propose, in the first place, to build the masonry of the abutments as far back as the lines AB, CD, and in the particular manner shewn in the section.

Having carried up the masonry to the level of the roadway, I propose, upon the top of each abutment, to construct as many frames as there are to be ribs in the centers, and of at least an equal breadth with the top of each rib. These frames to be about fifty feet high above the top of the masonry, and to be rendered perfectly firm and secure. That this can be done, is so evident, I avoid entering into details respecting the mode. These frames are for the purpose of receiving strong blocks, or rollers and chains, and to be acted upon by windlasses or other powers.

Centering of deal baulk; in four separate ribs carried out from the abutments on each side, and supported by chains.

I next proceed to construct the centering itself; it is proposed to be made of deal baulk, and to consist of four separate ribs, each rib consisting of a continuation of timber frames, five feet in width across the top and bottom, and varying in depth from 25 feet, near the abutment, to 7 feet 6 inches at the middle or crown. Next to the face of the abutment, one set of frames about 50 feet in length can, by means of temporary scaffolding and iron chain bars, be readily constructed and fixed upon the masonry offsets of the abutment, and to horizontal iron ties laid into the masonry for this purpose. A set of these frames (four in number) having been fixed against the face of each abutment, they are to be secured together by cross and diagonal braces; and there being spaces of only 6 feet 8 inches left between the ribs (of which these frames are the commencement) they are to be covered with planking, and the whole converted into a platform 50 feet by 40. By the nature

of

of the framing, and from its being secured by horizontal and suspending bars, I presume every person accustomed to practical operations will admit that these platforms may be rendered perfectly firm and secure.

The second portion of the centering frames, having been previously prepared and fitted together in the carpenter's yard, are brought in separate pieces, through passages, purposely left open in the masonry, to the before-mentioned platform; they are here put together, and each frame raised by the suspending chain-bars and other means, so that the end, which is to be joined to the frame already fixed, shall rest upon a small moveable carriage: it is then to be pushed forward, perhaps upon an iron rail-road, until the strong iron forks which are fixed upon its edge, shall fall upon a round iron bar which forms the outer edge of the first or abutment frames: when this has been done, strong iron bolts are put through eyes in the forks, and the aforesaid second portion of frame-work is suffered to descend to its intended position by means of the suspending chain-bars, until it closes with the end of the previously fixed frame, like a rule joint. Admitting the first frames were firmly fixed, and that the hinge part of this joint is sufficiently strong, and the joint itself about 20 feet deep, I conceive that even without the aid of the suspending bars, that this second portion of the centering would be supported; but we will for a moment suppose, that it is to be wholly suspended.—It is known by experiments, that a bar of good malleable iron, one inch square, will suspend 80,000lbs. and that the powers of suspension are as the sections; consequently a bar of 1½ inch square will suspend 180,000lbs. but the whole weight of this portion of rib, including the weight of the suspending bar, is only about 30,000lbs or one-sixth of the weight that might be safely suspended; and as I propose two suspending chain-bars to each portion of rib, if they had the whole to support they would only be exerting about 1-12th of their power; and considering the proportion of the weight which rests upon the abutments, they are equal also to support all the iron-work of the bridge, and be still far within their power.

Placing of the second portions of the frames

Having thus provided for the second portions of the centering a degree of security far beyond what can be required, similar operations are carried on from each abutment, until the

and the others.

parts are joined in the middle and form a complete centering; and being then braced together, and covered with planking where necessary, they become one general platform or wooden bridge on which to lay the iron-work.

Upon this centering the arch itself is laid.

It is, I presume, needless to observe, that upon such a centering or platform the iron-work, which is understood to have been previously fitted, can be put together with the utmost correctness and facility; the communications from the shores to the centering will be through the before-mentioned passages left in the masonry.

Main ribs and framed connection at top.

The form of the iron-work of the main ribs will be seen by the drawings to compose a system of triangles, preserving the principal points of bearing in the direction of the radius. It is proposed in the breadth of the bridge (i. e. 40 feet) to have 9 ribs, each cast in 23 pieces, and these connected by a cross-grated plate, nearly in the same manner as in the great Aqueduct of Pontcysylte over the valley of the Dee, near Llangollen; the fixation of the several ribs in a vertical plane appearing (after the abutments) to be the most important object in iron bridges, I propose to accomplish this by covering the several parts, as they are progressively fixed, with grated or reticulated and flanched plates across the top of the ribs. This would keep the tops of the ribs immovable, and convert the whole breadth of the bridge into one frame; besides thus securing the top, I propose also having cross braces near the bottom of the ribs.

Consideration of the pressure.

The main ribs being thus fixed, covered and connected together, the great feature of the bridge is completed; and as, from accurate experiments made and communicated to me by my friend the late William Reynolds, of Coalbrook Dale, it requires 448,000lbs. to crush a cube of $\frac{1}{4}$ inch of cast iron of the quality called gun metal, it is clear, that while the ribs are kept in their true position, the strength provided is more than ample.

Method of easing the centering; and general advantages of this general process.

When advanced thus far, I propose (though not to remove) yet to ease the timber centering, by having the feet of the centering ribs (which are supported by off-sets in the masonry of the front of the abutment) placed upon proper wedges; the rest of the centering to be eased at the same time by means of the chain bars. Thus, the hitherto dangerous operations of

striking

striking the centering, will be rendered gradual, and perfectly safe: insomuch, that this new mode of *suspending* the centering instead of *supporting* it from below, may, perhaps, hereafter be adopted as an improvement in constructing iron bridges, even in places not circumstanced as are the Menai Straits. Although the span of the arch is unusually great, yet, by using iron as a material, the weight upon the centre, when compared with large stone arches, is very small; taking the mere arch stones of the centre arch of Blackfriars bridge at $156 \times 43 \times 5$ equal to 33,540 cubic feet of stone, it amounts to 2,236 tons, whereas the whole of the iron-work in the main ribs, cross-plates and ties, and grated covering plates, that is to say, all that is lying on the centering at the time it is to be eased, weighs only 1,791 tons; it is true, that from the flatness of the iron arch, if left unguarded, a great proportion of this weight would rest upon the centering; but this is counterbalanced by the operation of the iron ties in the abutments, and wholly commanded by the suspending chain-bars.

Intermediate supporters of the roadway framed triangular.

When the main iron ribs have been completed, the next step is to proceed with the iron supporters of the roadway; and these, instead of being constructed in the form of circles, or that of perpendicular pillars, as hitherto, are here a series of triangles, thus including the true line of bearing. These triangles are, of course preserved in a vertical plane by cross ties and braces: iron bearers are supported by these triangles, and upon the bearers are laid the covering plates under the roadway, which instead of being solid are (in order to lessen the weight) proposed to be reticulated.

If I have throughout this very succinct description, made myself understood, it will I think be admitted, that the constructing a single arch across the Menai, is not only a very practicable but a very simple operation; and that it is rendered so, chiefly by adopting the mode of working from each abutment, without at all interfering with the tideway.

II.

METEOROLOGICAL JOURNAL.

1813.	Wind.	Barometer. Max.	Min.	Med.	Thermometer. Max.	Min.	Med.	Evap.	Rain	
2d Mo.										
Feb. 22	S W	29·80	29·69	29·745	57	41	49·0	—		☽
23	N W	30·03	29·8[illegible]	29·915	45	35	40·0	—		
24	W	30·14	30·03	30·085	49	32	40·5	—		
25	S W	30·13	29·83	29·980	50	35	42·5	—	0·71	
26	S W	29·90	29·70	29·800	52	35	43·5	0·50	8	
27	N W	30·33	29·90	30·115	46	32	39·0	—		
28	N W	30·36	30·30	30·330	50	34	42·0	0·23		
3d Mo.										
March 1	S W	30·30	30·20	30·250	51	39	45·0	—		●
2	S W	30·20	30·02	30·110	47	35	41·0	—	—	
3	Var.	30·33	30·02	30·175	52	32	43·0	—	—	
4	S W	30·33	30·20	30·265	51	36	43·5	—	6	
5	W	30·34	30·20	30·270				—		
6	W	30·40	30·34	30·370	53	35	44·0	—		
7	N W	30·40	30·30	30·350	49	39	44·0	—		
8	N W	30·30	30·20	30·250	54	43	48·5	—		
9	N W	30·20	29·89	30·045	52	36	44·0	0·56	—	☾
10	E	29·96	29·89	29·925	42	26	34·0	—	0·12	
11	N E	30·21	29·96	30·085	39	24	31·5	—	—	
12	N E	30·27	30·21	30·240	37	24	30·5	—		
13	N W	30·27	30·20	30·235	40	29	34·5	—		
14	S W	30·20	30·10	30·150	47	40	43·5	0·19		
15	S W	30·18	30·10	30·140	53	43	48·0	—		
16	Var.	30·18	30·09	20·135	51	32	41·5	—		
17	N E	30·09	29·96	30·025	56	32	44·0	—		○
18	N W	29·96	29·96	29·960	58	36	47·0	—		
19	E	29·96	29·78	29·870	58	40	49·0	—		
20	S W	29·96	29·78	26·870	56	35	45·5	—	0·14	
21	S W	29·96	29·84	29·900	53	42	47·5	0·31		
22	S W	30·25	29·96	30·105	55	33	44·0	—		
23	N W	30·30	30·28	30·170	50	34	42·0	—		
24	W	30·28	29·98	30·130	47	39	43·0	0·18	0·35	
		30·40	29·69	30·109	58	24	42·50	1·97	1·46	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Second Month. 24. Hoar frost. About 6, p. m. a very dark cloud came over, lowering with an arched base, as before thunder, and presently discharged a shower of large hail and rain, which was accompanied with a cold wind. 25. Fair, a. m. wet and windy p. m. and night. 26. The same. 27. *Cirrus, cumulus,* and *cirrostratus* clouds appeared together: much wind: about 7, p. m. wind N. W. A bright meteor passed from the zenith towards the N. declining a little westwards. 28. Clear morning: wind moderate.

Third Month. 1. Hoar frost, fair. 2. 3. Light showers. A *Nimbus* appeared S. of the setting sun in the 3d. which went away Southward. 9. Light showers. 10. a. m. sleet. At sunset, a *Cumulostratus,* with a snowy appearance: some hail-balls in the night. 11. A Nimbus was perceptible by 7, a. m. forming in the N. E. There were some heavy (though transient) squalls of snow during the day. Abundance of snow fell, on this and the following night, to the Southward, extending as far as the coast of France.

RESULTS.

Prevailing winds Westerly, with a marked interruption by a current from the N. E. occasioning snow about the middle of the period.

Barometer: greatest observed height, 30·40 in.; least 29·69 in. Mean of the period 30·109 inches.

Thermometer: greatest height 58°; least 24°; Mean of the period, 42·50°.

Evaporation 1·97 in. Rain, &c. 1·46 in.

L. HOWARD.

TOTTENHAM,
Third Month, 25, 1813.

III.

Note by LUKE HOWARD, *Esq. on the Meteors commonly called Shooting or Falling-stars, in reference to the Letter from John Farey, Sen. Vol.* XXXIV. *p.* 298.

THE name of *falling-star* appears to me to have been applied to these meteors, in consequence of their being observed to *descend* from the place of their first appearance towards the horizon. This is their usual apparent motion, the exceptions, found in large meteors, traversing a great extent of horizontal space, may possibly admit of an explanation which should reduce the whole to the class of falling bodies, or of projectiles analogous to the sky-rocket; not that I suppose any considerable proportion of them to reach the earth in a state of aggregation, but rather that they are commonly dissipated in the lower atmosphere.

My observations on these phenomena have not hitherto had the advantage of previous concert with another observer at a distance: but, such as they are, they tend unequivocally to the conclusion, that igneous meteors in general belong to our own atmosphere; and come under the class of effects which we call electrical. Should John Farey, Sen. or B. Bevan, be willing to favour us, and the public, with the detail of their observations made in concert on meteors, they may be gratified in turn with the reasons on which this conclusion is founded.

L. HOWARD.

Tottenham,
Fourth Month 10, 1813.

Remarks

IV.

Remarks and Elucidations of the Methods of Computation used by the American Boy, Zerah Colburn. In a Letter from Mr. S. Ellis.

To Mr. Nicholson.

SIR,

THE communications of the Philosophical Journal show, that many persons have not a notion of any fixed rule by which the calculating boy conducts his arithmetical operations. But I cannot accede to the opinion of those gentlemen who suppose that the process is unknown to the most intelligent of our great mathematicians. It is probably comprehended by them all; though an explanation of it has not yet been presented to the public. The learned and scientific cannot devote their days to the developement of subjects merely curious, and not possessing any claim to utility. Their pursuits are directed to objects of more importance; and the computing boy had not to apprehend, that any celebrated mathematician would take the trouble to invalidate his pretensions to a rule, distinguished only by a novelty of application, but having no great novelty of character, and no character of utility. The humility of my talents excluding me from all avocations of real consequence, I shall endeavour to explain those numerical operations which have failed to excite the investigations of the more capable and proficient.

On numerical computations, particularly those performed by Z. Colburn.

Every one will readily concede to the young calculator his just portion of merit. The mental qualities essential to a good memory were native and his own; his original faculties of comprehension may have been uncommonly vigorous and acute; and the distinguishing habits of his infancy may have reflected indications of an intellect peculiarly susceptible of that mechanical intelligence, which has been communicated to him at this early period of life. The leading facilities to extraordinary attainments, were, perhaps, spontaneously exhibited; and the singular direction that has been given to his juvenile studies, evinces the confidence which was entertained of his intel-

On numerical computations, particularly those performed by Z. Colburn.

intellectual competency to execute the instructions of his tutor.

It will be proper to separate the consideration of the boy from the examination of his studies; and I shall hope, that a concise inquiry into these will not be erroneously denominated a personal severity against the student.

The attention of the infant mind is invariably excited by sensible objects around it. Any thing that pleases the eye, or that delights the ear, may engage the spontaneous preference of a child; and the feelings having been once peculiarly roused by a favourite object, the momentary charm may ripen into a permanent attachment, and all the faculties of the understanding become gradually absorbed by the contemplation of that object alone. In the property of numbers there is not any thing which can appeal to the untutored senses. Every thing is recondite and imperceptible. Numbers are themselves artificial, the slow production of very great men, who had real occasion for extensive and intricate combinations. It was a conviction of their utility that stimulated Genius to invent them; and not any quality in themselves to allure and gratify the senses. The first contemplation of numbers is unpleasant and irksome; and much laborious application is necessary to produce even an idea of those latent properties which render them so eminently useful. All the qualities of numbers are precisely the reverse of those which are known to operate attractively on the infant mind; and the statement is not credible, that the young calculator has derived his modes of computation from the unassisted efforts of his native faculties. He has certainly been assiduously instructed in the limited use of figures; and his whole knowledge of them is probably derived from that instruction. In the supposed methods of calculation, there will be found very little of the ingenious or the novel; but there is ingenuity and novelty in exhibiting their effects to the world as the natural fruits of ungrafted genius, or the voluntary attainments of an uneducated child. To estimate the following explanations, it will be requisite to bear in recollection a few relative circumstances; viz. that in the science of analysis, every operation seems difficult and complex till maturely comprehended; and that the demonstration of of a theorem has more the appearance of intricacy than the rule resulting from it will be found to possess in practice.

On numerical computations, particularly those performed by Z. Colburn.

The modes of multiplying four figures by four, and of extracting the square and cube roots, are all of them founded upon the principles of involution and evolution; and by these shall my proceedings be governed. In what way the factors are discovered, which enter into the composition of a large numerical quantity, and by what rule prime numbers are ascertained, I shall not pretend to examine. Upon this subject, and indeed upon all others connected with the more important properties of numbers, much valuable instruction may be obtained from the elaborate elements of the ingenious Mr. Barlow.

To square 7654 by the common rule, without recording the several operations, would require a memory more perfect than the human faculties are generally thought capable of attaining; although, by a peculiar discipline from infancy, the memory is capable of being improved to a degree of extraordinary retention. The calculating boy is a surprising evidence of this fact; and it becomes a reasonable purpose to search for those expedients which can lead his efforts with most ease to the accomplishment of his daily exhibitions. Even with the assistance of all artificial aids, his occupations demand a very cultivated and retentive memory; but, as some fixed rules are positively used by every calculator, whose results are uniformly correct, we have only to discover those which have the fairest character of simplicity, and consider them as his guides.

To establish the probability, that the subsequent methods of calculation resemble those which have been prescribed to the computing boy, I shall mention a few peculiarities that characterize the operations. The young calculator can evolve the cube root of twelve figures, where only four of them are made known; he can detect an imperfect cube, and tell the nearest root to the quantity given. These three singularities belong to the following rules. The multiplication of four numbers into each other, and the extraction of the square and cube roots are conducted upon principles exactly similar, which similarity diminishes the application requisite to understand them, and imposes upon the memory less efforts of retentiveness,

On numerical computations, particularly those performed by Z. Colburn.

tiveness than if the principles were essentially different in all the various operations.

The same letters shall be used in all the calculations, that the similarity of these may be rendered more perceptible; and I shall, therefore, in multiplying four figures by four, employ only a single set. Those who wish to pursue the method more in detail, can readily change the letters of one line, and by that manner arrive at the desired variation of the rule.

7654 multiplied by 7654 = 58,583,716.

Produce the square of 4 =	1	6
Add twice 5 × 4	40	
	4	1
The square of 5	25	
	29	
Twice 6 × 4	48	
	7	7
Twice 7 × 4	56	
	63	
Twice 6 × 5	60	
	12	3
The square of 6	36	
	48	
Twice 7 × 5	70	
	11	8
Twice 7 × 6	84	
	9	5
The square of 7	49	
	58,583,716.	

Another

On numerical computations, particularly those performed by Z. Colburn.

Another Method.

Produce 765 × 4	3060
Add 76 × 5	38
Double this sum	41060
	82,1,20
Add the square of 4..........	16
square of 5..........	25
square of 6	36
	11.83716
Twice 7 × 6................	8,4
Square of 7	49
	58·583·716

As far as the functions of the memory are regarded, the advantages of the rules exhibited will be obvious without much explanation. There need be only two lines of two figures produced and added together in any one term. The right-hand numbers dashed off, constitute the quantity required, and the proper disposal of the successive additions will become perfectly easy by a very little practice. Let m represent 7, n 6, p 5, and q 4; connectedly with their local values; then $m+n+p+q$ multiplied by $m+n+p+q$, will produce
$m^2+2\,m\,n+2\,m\,p+n^2+2\,n\,p+2\,m\,q+2\,n\,q+p^2+2p\,q+q^2$
Therefore the square of $m+n+p+q$, or of 7654, is as follows:

m^2 =	49·000·000
$2mn$........	8·400 000
$2\,mp$	700·000
n^2........	360 000
$2\,np$	60·000
$2\,mq$	56·000
$2\,nq$	4·800
p^2........	2·500
$2\,pq$	400
q^2.........	16
	58,583,716

An

On numerical computations, particularly those performed by Z. Colburn.

An illustration is here afforded of the combinations by which the product arises from two quantities multiplied into each other; and a familiar acquaintance with the preceding rules will render the extraction of the square root a process of easy attainment.

To find the square root of	58·58·37·16
Deduct the square of the first root 7=	49
	95
Deduct twice $7 \times 6 = 84$; and $6^2 = 3,6$—say	87
	8
Deduct twice 76×5, say 15×5	75
	5
Again, the same divisor, 15×4	6,0

As the last root is known to be a 4 or a 6, it must obviously be 4, which gives the nearest product to the quantity wanted. If the second 8 had been operated upon, the 4 would have been produced correctly: but, to shorten the calculations, this nicety may be omitted.

The more full explanation is the following:

1. Deduct the square of the first root, 7.

2. Multiply twice the first root by the number, which will bring the product nearest to the term brought down; square also the assumed number; subtract the two sums from the dividend. The new quotient, 6, will be the second root.

3. Then twice the first and second roots (placed in their local order thus 76) must be multiplied by the number that will raise them nearest to the remainder of the last dividend, considered in its local value. This new factor, 5, will be the third root.

4. Again, twice the first and second roots, 76, multiplied by the number, which will produce a quantity nearest to the remaining dividend, will shew the fourth root, 4.

It is only in the operation, where the second root is found, that any nicety is required. The multiplier (of twice the first root) may occasionally be assumed an unit too much; but the error,

error, whenever it can happen, becomes immediately obvious; and, by taking the relative quotient a single unit less than the fallacious multiplier, the second root will be always obtained correctly. This incident may unfrequently retard the calculation for a moment; but ought never to defeat the accuracy of the final result. On numerical computations, particularly those performed by Z. Colburn.

Example.

	Square	59·05·92·25
Deduct square of the first root 7		49
		100
Twice the first root, 7 × 6 = 84		
Square of 6 - 36		87
		13
Twice the first and second roots	76 × 8	
say	15 × 8	12
		1

5 must be the fourth root without any trial, 7685 is therefore found to be the root of the given square. In the second operation twice 7 will go 7 times in 100, which is the case of nicety alluded to; but it must be instantly perceived that the square of 7 added to 98 would raise the sum too high for the dividend of 100; therefore, the next lower digit is adopted; and the very first substitute will always be right, as the same divisor is employed in the third and fourth operations (where both are necessary) the work is completed with expedition and accuracy.

Practical

On numerical computations, particularly those performed by Z. Colburn.

Practical illustrations of the rule.

The square of $m+n+p+q$, or 7654, is 58·58·37·16

Deduct m^2 - - - - - -	49
	958
Deduct $2\,m\,n + n^2$ - - -	876
	82
Deduct $2\,(m+n)\,q$ - - -	76
	637
Deduct $2\,(m+n)\,q$ - - - -	608
	29
The root is already found; but to prove correctness, deduct p^2 - - - - -	25
	416
Deduct $2\,p\,q$ - - - - - - -	400
	16
Deduct q^2 - - - - - - - - - -	16

The involution and evolution of similar numbers have only this difference; one process consists of repeated multiplications and additions; the other chiefly of multiplications and subtractions. One extends the length of a chain, by adding link to link in progressive order; the other diminishes the length, detaching link from link by retrocessive reductions.

To extract the cube root of - -	448·399·762·264
Deduct the cube of the first root 7 -	343
	,105
Deduct 3 times 7^2 say 15 and 3 times, $7\times,6$ say 1,2—say 16×6. - - - -	96
	9
Deduct 1,2 (already found) $\times,5$—,60 and the cube of ,6 say ,2 - - - - - - -	,8
	8,2
Deduct 16 (already found) $\times,5$ - - -	80

The

The unit figure of the cube being valued by inspection, the root is already found to be 7654.

On numerical computations; particularly those performed by Z. Colburn.

Explanation 1st. Use only three figures of the cube; often two will be sufficient. The first root is known by inspection, and subtract its cube from the left-hand period.

2d. Then the second root is to be assumed as correctly as the judgment can guess, taking it rather above than below the real value; precision is not required, and very little practice will make this part of the process quite easy. Multiply the assumed figure by the first root; add the product to the square of the first root; and three times this last sum must be multiplied by the number that will bring it nearest to the remaining dividend. This last factor will be the second root.

3d. Deduct three times the product of the first and second roots multiplied by an assumed figure that you may conceive to be the next root wanted; and deduct also the cube of the second root. In this stage, the divisor, which has been used in the second operation (there found to be 16) will give the third root correctly.

The fourth root is known by inspection. The cube of the second root may be neglected in most cases; and always when the root is below 5; only the left-hand figure of the cube is required.

After a little exercise, all the divisors are readily found; because each of them has been obtained in the second operation, and there becomes known. The chief difficulty attends the explanation, to render the process intelligible.

Illustration of the rule.

Cube of $m+n+p+q$—or of 7654 = 448·399·762·264

Deduct m^3 - - - - - - - -	343
	105
Deduct $3\,(m^2+m\,n)\,n$ - - -	95·760
	9·639
Deduct $3\,m\,n\,p$ and n^3 - - - -	846
	8·793
Deduct $3\,(m^2+m\,n)\,p$ - - - -	7·980
	813·762·264

 The

On numerical computations; particularly those performed by Z. Colburn.

The root 7654 is already found, as the fourth root is known by inspection; but, to prove correctness,

Deduct	$3\,m^2q$	588
	$6\,m\,n\,q$	100·8
	$3\,n^2p$	54·0
	$3\,m\,p^2$	52·5
	$6\,m\,p\,q$	8·4
	$3\,n^2q$	4·32
	$3\,m\,q^2$	·336
	$3\,n\,p^2$	4·500
	$6\,n\,p\,q$	720
	$p\,3$	125
	$3\,p^2q$	30
	$3\,n\,q^2$	28·8
	$3\,p\,q^2$	2·4
	q^3	064
		—— 813 762·264

It will be perceived that, in multiplying four figures by four, and in extracting roots, the rules are derived from the same property of numbers; and any person, who can accomplish the first operation, may be qualified in a day to effect the other two.

I am inclined to believe, that the preceding methods are employed by the calculating boy; and that he has attained them exclusively from the laborious and instructive attentions of a preceptor. To learn, and to apply them, does not require faculties more intelligent than those which are exhibited by other children in the kingdom. The combination of words, in the most simple language, is fifty times more complex; and so is the modulation of varied tones, which express the several feelings; yet these things are mechanical; and every child attains them by observation and practice, without knowing the scientific rules by which his expressions are regulated. If the proper instruction is early communicated to the infant mind, the use of figures may be gradually rendered as familiar as the letters of the alphabet, or the words of a vocabulary.

I conclude, agreeably to my preceding sentiments, that the capabilities of the calculating boy are not the indigenous shoots of uninnoculated genius, but the slowly matured fruits of instruction

instruction and prescribed habits of exercise. The exhibition of them is, however, novel and interesting, and wonderful in a child.

S. ELLIS.

London,
April 14*th*, 1813.

V.

On the Formation of the Seeds of Plants, and other Objects, relative to their Structure. In a Letter from Mrs. AGNES IBBETSON.

To Mr. Nicholson.

SIR,

I HAVE now to announce one of the greatest discoveries I have yet made in Phytology; one, likely also to approximate us so nearly to the true delineation of the general formation of the vegetable world, as at last (I flatter myself) to leave no doubt in the minds of the most incredulous; for, in this case, to *look* is to *find*, and those who will labour with me, by pursuing the same path, and dissecting the same specimens, will receive the same gratification, in the conviction of these important truths. They require only *habit, good eyes,* and *instruments,* to behold them, and some *ability* in dissecting, to be satisfied of the reality of all I have shewn. To prove the history of botany, by means of dissection, is, indeed, *an important* and *difficult process*; as it is only *by degrees* the whole can be developed; many mistakes will inevitably occur. But, if the general foundation of the arrangement is but acknowledged to be just and true, we shall afterwards, by dissection, easily manage the minor parts: finishing them alternately in a more perfect style, till the whole forms one grand display of nature, organized by the *power divine,* and shewing *discriminations* and *consistencies,* which *his* arrangements alone can plan; *his wisdom alone can produce.* It is to this exactitude of delineation I trusted; I was well assured, that if I *deviated not* from what

Discovery announced.

General foundations.

dissection presented to me, neither *exaggerating, nor adding of my own*; I might with perfect security depend, that *all* would agree at last; all assimilate together, and form one grand picture, astonishing as beautiful.

This last discovery, from linking all the divisions, perfects the foundation; and though, in some few parts, it may, at first, appear rather complicated, yet, in the general review, there is a beautiful simplicity, that makes all plain and easy to be understood.

Formation of seeds in the root.

The discovery I have to announce, is the formation of seeds in the root; for it is a certain truth, that, from the wheat to the cedar, from the moss to the largest forest tree, all plants form their seeds in the root only. When I say the seed, I mean only that essential drop which joins to the vital string, and forms the essence of the seed; and afterwards composes the *chief part* of the *corcalum*. Almost thirteen years of the most intense application, devoted to the dissection of plants, must have given me some insight into their formation; especially as I have made it a rule never to terminate with a plant, till I have exposed every part (every different vessel) to examination, in the solar or double compound microscope. And yet one matter has all that time puzzled me in such a manner, that but for the regular gradation by which I have been led up to it, through the herbaceous plants, I should, *never probably*, have been able to *solve the mystery*. The finding these balls in the alburnum vessels now and then, and their complete disappearance, as soon as I began generally to seek them, convinced me that it was a disorder or a criptogamia. But, when dissecting herbaceous roots, I found *flowers* as well as *seeds* in them; and that the pollen was not arranged in their cases, or even made into balls, till it arrived at the top of the plant. I then began to conjecture, whether the seeds also might not be formed separate, and be the very balls I had so often found in the alburnum, fixed on longitudinal vessels. But I was still resolved, not even to hint at the fact, till I had watched two or three seasons to ascertain it, and to prevent the possibility of mistake: this I have now done, and can announce the discovery with the most perfect conviction. The flow of the sap in the alburnum vessels, brings the seeds twice each year, at the barking time, from the root; (for this is the season in which the new row of wood is formed,) and

Through herbaceous plants.

Difficulty of understanding the alburnum.

and that the bark retires back for the purpose, as I showed in a former letter. Whether the bark is pushed back by the violence of the rising sap, (which I am inclined to think,) or that it retires back of itself, the effect is still the same. The sap rises between the two cylinders of the wood and bark, but I must observe, that this process is wholly separate from the usual flow of the sap in the wood, and belongs to the *alburnum vessels only*. The liquid is not wholly *loose*; vessels, composed of a narrow-twisted band, produce the exterior of the cylinder, and this is the cause of the various changes observed in the appearance of the *alburnum*: sometimes it looks like tiles laid on each other; sometimes like a twisted vessel, laid almost flat; but each varying form is owing to the *pressure of the sap* on the divided foldings of this curled ribbon. It must be remembered, that it is a vessel within a vacancy; therefore, the sap is doubly secure: but this very circumstance causes the jelly-like matter to assume folds and creases, difficult to understand, till the cause is known. When the sap has, in its liquid state, deposited all the seeds, and lodged them in the buds, then by degrees having cleared itself of them, it coagulates, and becomes a jelly. In this state it remains *all the winter*, and when the spring arrives, and the bark begins to retire, the bud of the preceding year completes itself, by forming its calyx and flower stalk, which it never does till the seeds have entered the corolla. But before the flower developes, another process takes place, which I might also announce as a late discovery, having only hinted at it before. It is the producing the pollen. When the corolla is in its cradle, the cases of the stamen are there, but no pollen. The powder of the males is formed by some mixture, which requires the dews to perfect it, some liquid from above, which is deposited on the top of the plant, conveyed into the interior by the hairs, and thus communicated to the higher juices. Take a branch of any tree, before it flowers, when the buds are still at the bottom of the peduncle, and cut open the whole, dividing it down the middle, and a yellow matter will be found at the top of the wood, where the pith stops, whose tainted appearance indicates something extraordinary; place a very thin slice of this in the solar microscope, and the yellow matter will be discovered to be the *pollen*, *protruding* in a distinct manner: nor is this all; the flowers, after being aggregated, begin to divide, then

The forming the pollen.

then the pollen is seen separating, and running through the pith into each flower, and thus attaining the stamen, as the seeds do through the alburnum; but the filling the stamen cases is always the last process. Thus both seeds and pollen are formed out of their cases, and yet they will all rise together to the developement of the flower, with the same admirable precision. It is only watching year after year, and continual dissections, that could have thus by degrees enabled me to discover every part of this *complicated formation*, but the many thousands of plants I have investigated *this year only*, and the excessive labour I have gone through, did, perhaps, merit some recompence, and I am most thoroughly repaid by the certainty of the acknowledged fact.

Both seeds and pollen formed out of their cases.

No seeds in the top root.

In trees I have not yet been able to trace the seeds lower than in the root, and not, as in herbaceous plants, in the strings. I showed in a former letter, that in the under part of the root of trees, a quantity of alburnum was laid up for future nourishment; but the seeds are found in one line only of this matter. I have never yet been able to trace them in the top root; indeed, if ever found there, it is difficult to catch the proper moment of seeing them; because, when the sap is quite liquid, it escapes, and the seeds with it, and it is only when it begins to form a jelly round the remaining seeds, that they can be thoroughly discerned and examined. But, in herbaceous plants, indeed in all that rise each year from the earth, the seeds are to be found at most times, and exhibit themselves in so very conspicuous a manner, that no person who will seek them can doubt their identity.

One year's growth of a tree.

In order perfectly to explain the process, (though I cannot do it without some repetitions recounted in former letters,) yet I will venture, for the thorough elucidation of the whole, to give a complete history of the formation and progress of a tree for one year, and as in this respect there are but two sorts of growth, the stem that does not lie down in the winter, and the stem that does; and that the former embraces trees, shrubs, and semi-shrubs; and the latter, all plants that rise each year from the earth. By giving the life of a tree for one year, and that of an herbaceous plant for the same time, I shall show, in a small compass, the whole progress of botanical vegetation. The first process in a tree is the formation of the flower bud on the line

of

of life. The vital part of a tree is that cylinder of vessels, which lies between the wood and the pith. This is the most important set of vessels in a plant, is that which in the alburnum ties the seeds together, and which former authors have called by the name of the impregnating duct, when appearing in *the seed.* It is always the source from which proceeds every important part of a plant, *seeds, buds,* and *radicles.* When the flower is first engendered next the pith, it is by means of a knot on this line, nearest to where it is to appear on the stem. A quantity of alburnum is then formed round it, the knot is broken, a bud shoots at each end, and the wood is prepared from within the plant, for the exit of the buds, by raising some vessels, and depressing others, and thus forming a covered way in the wood, through which they pass across this apparently hard and close matter, till they reach the cradle, already formed in the bark for their reception, being that screw before described. It is scarcely possible to conceive a more beautiful and wonderful process, or one more easily comprehended. It requires only stripping off the rind and bark from any branch of a tree, from February to May, and loads of buds will present themselves. How it could escape the observation of botanists to this time, appears to me to be an enigma. When all this is completed, and the bud is lodged in the screw, it there remains till the general flow of the sap brings with it the seeds, when it has in its liquid state deposited them in the various buds, then by degrees, having cleared itself of them, it coagulates, and becomes a jelly. In this state it remains all the winter, and when spring approaches, the flowers, which were now aggregated and *en masse, separate,* and each forms its calyx and flower stalk, which it never does, till the seeds have entered the corolla. When the flower has expanded, the liquid in the line of life dilates, and fills both nectaries, and rises each day, at the greatest heat, to the top of the pistil, and descends again within the tube, when the cold returns, till the pollen is ripe. At last the pistil receives the powder, and its juice dissolves it, carrying the mixed liquid down the cylinder to the heart of each seed, *through* that very pipe by which the seeds were tied in the alburnum. The seed, or rather that drop of jelly that has proceeded from the root, that clear and vital drop, which, with the string, forms the life of the seed, is now in its first and original state;

The line of life the most important vessel.

Buds passing through the wood.

Remains all the winter.

Filling the nectaries.

state; but no sooner do the juices of the stamen and pistil meet and touch this vessel, than the corculum begins to grow, and its progress from *that moment* is rapid. The next process is the flowing of the juices of the nectary, which form various temporary vessels, running into the seed, while they still continue joined to the flowers; but, as soon as divided from it, the nourishing vessels, already thoroughly filled, enter the seeds from the bark, and form, with a part of the interior before composed, a milk to support the young plant. Then the cotyledones begin to grow; but, so far are they from being capable of affording aliment to the embryo, that they themselves require more nutrition than any other part of the plant; nor would they, in case of bestowing nutriment, fail to decrease, as the spot in the seed does, which, from yielding so much matter, becomes a circular vacancy; whereas, the cotyledones are peculiarly plump and thick to the last moment. But it is certainly a mistake to suppose they nourish the embryo, for as long as the seed defers opening, (on account of cold, or any other cause), the cotyledones increase in number; in a walnut I have found from three to nine, and though, because lying in a case, in which the two projecting ones alone can appear, it has been thought to have only two, yet if the box is opened, many more may be seen attached to it, and growing from it. No one could admire Jessieu's plan more than myself; it had, indeed, but one fault,—the not being true. When the cotyledones are completed, the embryo strong and firm, and the heart thoroughly arranged, the radicle, or rather the root, begins to grow, but not till after the seed is again placed in the ground. This addition, by overfilling the seed, quickly bursts it at the top, and the root shoots out, turning downwards, endeavours to reach the earth, which it quickly does; while the embryo coming forth at the same place in the seed, rises into a stalk, and developes its leaves; then the flower bud of the next year begins again to form on the line of life, and when the flow of the sap has taken place, lodged the seeds in the new buds, where they will remain the winter, and the flowers of the present year have expanded; the stem begins to prepare for the shoot of the next year, by the formation of those screws in which the buds of the ensuing season take refuge, and remain their allotted time, each in its cradle. The screw forms one shoot between

Cotyledones increase in number.

Formation of the buds of the next year.

between bud and bud, for they draw out between each, like a telescope, and thus complete by degrees their preparation for the following year. The screw appears to be that species of formation which Nature adopts on every important occasion, *in bola*, as I shall show when delineating the herbaceous plants. I have now depicted the whole dissection of plants, that do not lie down in the winter; I shall next explain the formation of those which do, and which rise each year from the earth. They are so different from trees, that nature appears to have formed them in a contrary mould; since the laboratory of trees is at the bottom of the flower stalk, but in herbaceous plants at the lower part of the root. Here not only seeds, but flowers also, are composed, and every decoction necessary to their production. As the corolla and stamen cases are both formed of the wood, no other juices being approximated sufficiently for the purpose. The calyx and peduncle are not made till the flower reaches the screw or axilla of the leaves, where the juices of the inner bark can attain them, to produce the green part of the flower, and the pollen is formed by the addition of the dew, as in trees. If a plant is taken, when only six or eight inches high, (suppose one of the pentandria digynia tribe), if cut the length of the stem and root, the flowers will be found mounting in large wood vessels, formed for the purpose, and passing through the middle of the root, and on both sides of the stem, and therefore easily distinguished, while the seeds coming even from the stringy roots, and thence entering the alburnum vessels, (which run through the root in various directions,) thus mount to the stem, while the flower has, in the mean time, ascended the plant, formed its calyx and stalk, and has only to run up the peduncle, to complete its formation. I know no plant that so admirably shows the seed arranging in the seed vessels, as the arum. If the plant is divided longitudinally, root and stem, a very thin slice, and cut exactly even, placed in the solar microscope, and the flowers already aggregated, the seeds will be seen mounting the alburnum, and arranging themselves one by one in the cases, and if *quite fresh*, they will continue to do so for nearly an hour after they are dug up and dissected; and it is certainly a most beautiful subject for the solar microscope. But what may be said to complete and perfect the discovery of the seeds, is the dissection of the wheat; which not only forms the vital part of the

Formation of the herbaceous plant.

Stringy roots contain the seeds.

The arum arranging the seeds.

the seed in the root, but the whole of it; so that the heart can be taken out and examined, and of course no person, who understands the dissection, can doubt its identity.

Difference of the flow of the sap.

With respect to the flow of the sap in the alburnum vessels, it is not regular as in tress: but rises at different times, according to the length of period, or number of months the plant remains in existence: in some vegetables three rows of alburnum are discovered, in others only two; but in general there is, I believe, very little division between them, and the flow is pretty constant, and mostly brings the seeds with it; and they are to be found at all times. The alburnum appears to be formed in the same manner as in trees, by a twisted or curled ribbon, which runs up in a vacancy; but its wood is very different, being extremely loose and weak, and very little stronger than in its first state of alburnum, and therefore very watery; and it differs also from trees in another particular, its sap-vessels being real cylinders, and not vacancies. They also vary in the place in which their spiral vessels mount in the stem—in trees they are found in the last few rows of the sap vacancies, besides that which forms the wood. But in herbaceous plants they are discovered next the bark: and it was this very circumstance that persuaded me, (in the first years of study,) that they might be the returning vessels for the sap, as in all twining plants, and herbaceous also, there is a set of cylinders (perfectly white) which run next the alburnum, and might well deceive in this respect: but when I came to *dissect them*, (which I did in innumerable plants) I found that they were all spirals confined in their cases; but when taken to pieces, and greatly magnified, easily seen to be such. Perhaps cutting a pentandria digynia plant, in an almost decayed state, shews them better than any other, because they are then divested of the bark which surrounding, conceals them; and when the rind is taken off, and the cases severed, they remain quite exposed. In all these plants they turn round each leaf repeatedly. It would appear as if it was to gain for them that space necessary to make the spirals act weil, by having a *greater distance of wire to contract and dilate in.* In the plants just mentioned they form a sort of *basket-work* at the top of each leaf, which appears to give great strength to the leaf-stalk in moving to and fro.

Different place of the spirals.

Appearance of the spirals.

As

As specimens of the herbaceous roots are too large, and that it would hardly be possible to give them within the prescribed limits for prints in this Journal, I shall present a drawing of a wood vessel fig. 1, and an alburnum vessel with F, a closed one fig. 2, instead, to show how the flowers and seeds pass up the herbaceous plants from the root. So very quick is the passage of the buds up the wood cylinder, formed for the purpose, that if it is cut horizontally, and left for an hour or two, the flowers will peep up above the wood vessels, and form a kind of nosegay or flower pot. It has always a spiral twisted up in the interior of the vessel apparently to keep it open, and prevent its pressing on the flowers within. As to the roots of herbaceous plants, it is impossible not to be struck with the astonishing difference between them and that part in trees, &c. The beautiful mechanism of the *former* is so *admirable*, that the mind finds it difficult, at first, to understand and discriminate the particular parts, and it is only by comparing them with other sorts of roots, that I learned, at last, to comprehend the whole arrangement. The root is evidently the laboratory of these plants, in which every different ingredient is composed and concocted; and instead of the sameness and simple regularity of the root of trees, shrubs, &c. flowers, seeds, and spiral wire appear all in their allotted places; and though annuals and herbaceous plants differ, yet it is too little to make it worthy discrimination, in so *slight a sketch* as this; nor does the bulb vary enough to particularize it, except to mention, that unlike the annuals, &c. the flowers mount in one or two cylinders only, instead of a circular row of wood vessels. I have described the formation of the plants that rise eath; and shall finish my letter by adding a few miscellaneous facts that increase the clearness of the general picture.

Various specimens with prints.

The root the laboratory of herbaceous plants.

I have also another discovery to make, which overcomes the many difficulties which have appeared, such not only to other Phytologists, but myself also: I have said that when in trees, &c. the sap is cleared from the seeds in the alburnum vessels, it becomes a jelly, and then wood by the insertion of the different vessels, elongated from the neighbouring wood which also introduces the spiral wire, in every direction, to form the sap vacancies. Dr. Smith observes, in his excellent treatise on botany, that if wood is taken to pieces thread from thread, no

Vacancies instead of sap-vessels.

sap

Vacancies instead of sap vessels.

No returning sap vacancies in the bark.

The screw to protect the herbaceous root.

sap vessels will be discovered, I used to think it was because the cylinders were so fluted; as in botany small vessels almost always surround the large ones: but I was mistaken; I have now solved the mystery; they are *vacancies* not *vessels*; and this is the reason why in each horizontal layer of wood, the sap holes are worked back with spiral wire, just like a button hole, to prevent the interference of any part of the net or threads filling up the apertures intended for the current of sap. This will also evidently shew that now they are known, they are too plainly marked, and too exactly delineated not to strike the eye, if, therefore, there were any in the bark, that part could not be dissected (in the manner I have done it, *vessel* from *vessel*) without their being perceived. But there certainly are no returning sap vessels or vacancies in the bark; in showing which trees best exemplify the seeds coming from the root, I should have mentioned the *firs*: particularly the larch, which so admirably distinguishes the completion of both *seeds* and *pollen*; for as they are both formed out of their cases, and that the male and female flowers are separate, the essence of the seed is in the beautiful red flower seen to coagulate at its commencement, while the empty seed vessels are all arranged in their proper places, at a vessel projecting (See Fig. 6 ee.) to receive the seeds, which soon begin to mount, and in less than a week, (if in increasing ripe flowers are taken) will be found at last, all to have arranged themselves in the seed vessels according to the manner appropriated to the plant, and the coagulated quantity below will almost all have disappeared, and if the male flower is taken, the pollen will be found in the same manner to have collected from the middle of the stem, and arranged itself at the bottom of the flower, and will be all carried up, *not as in the seeds through a vessel like the alburnum*, but through *the middle* of the flower, to the stamen cases, which they enter. Both these processes are so plain, so easily seen, (now they are pointed out) that every one may behold them, with no very powerful magnifier, if they will cut the flowers through the middle, then take off an even slice; and the pollen and seeds being divided, makes it far more clear and absolute. I promised also to show that sort of screw which in herbaceous plants prevents the roots which are permanent, from being hurt by the settling of the water in the

the hollow of the stems. It is at once a most beautiful contrivance, and an admirable piece of mechanism, without which the roots would be ruined before the return of the following spring: The stem, when decayed, appears like a double cane, from the bark and pith having all melted away between each; these are often filled with water which, without the precaution of the screw, would spread the rot to the middle of the root; it is a double screw, one within the other, which finishes at the beginning of the root, and thoroughly protects it from rot and moisture, it breaks away when the new shoot is going to commence, which never begins at the same place.

I am, Sir,
Your obliged Servant,
AGNES IBBETSON.

Fig. 1. Plate II. A wood vessel taken from an herbaceous plant, shewing the flowers as they pass up to the top of the plant: AA the spiral which keeps the plant distended, that pressure may not hurt the flowers in the interior.

Fig. 2. An alburnum vessel; the same in both trees and herbaceous plants, conveying the seeds from the root to the top of the plant or to those fibrous stems which bear the flowers still in their aggregate state, into which they enter, lodging them in each separate flower. BB The seed are always arranged on the line as they are in the different seed vessels; but in far greater quantities than ever come to maturity. F The alburnum vessel closed.

Fig. 3. A double screw which separates the old stem from the herbaceous root, prevents the latter from being decayed and destroyed.

Fig. 4. The female flower of the larch; showing the manner in which, after mounting the alburnum vessels, the seeds are collected at the bottom of this part, and then dividing, and running again into the alburnum, mount to each separate bud and these enter the seed vessel. Fig. 6. One seed.

Fig. 5. Manner in which the seeds enter the seed vessels in the arum, CC seed vessels DD seeds.

A Memoir

VI.

A Memoir upon the Limits of Combustibility of Gaseous Inflammable Mixtures diminishing in Density, and upon the Colours of the Electrical Spark in different Mediums, by M. GROTTHUSS.*

Rarefied gases not combustible.

IN the ninth volume of Gehlen's Journal, I shewed how much the influence of the pressure of the air is of importance in the combustion of bodies, particularly with regard to the gaseous mixtures. I proved, that at the height of about 35,000 feet, all combustion in the atmosphere must cease. From my experiments of that time it may be concluded, that the combustibility of a gas, comports itself as the degree of dilatation at which it ceases to be inflammable. If we lay down this principle for measuring the combustion of inflammable gaseous mixtures, we shall find anomalies which are interesting, but difficult to be explained.

Remarkable difference between the combustibility of expanded compounds of oximuriatic gas, and those of oxigen.

II. It is generally admitted, that most combustible bodies are more easily inflamed at the ordinary pressure of the atmosphere in oximuriatic gas, than in oxigen gas. The oximuriatic gas produces a sudden inflammation by simple contact of phosphorus with different metals, and with certain gases; an inflammation which does not take place with oxigen gas, except when it has obtained a maximum of compression, which may happen, as I have shewn, from the united action of expansion and resistance. A mixture of oximuriatic gas and hydrogen gas inflames, by a slight heat externally applied, even by the solar rays; and from this fact it was to be presumed, that a gas so easily inflammable, would require a much more considerable dilatation to deprive it of the property of inflaming, than the denotating gas, composed of two measures of hydrogen, and one of oxigen, gas. This would be a consequence, however, which is contradicted by the following fact.

The former,

III. I filled a tube, (of which the internal part of its upper

* From a Translation of Vogel of the original Schweigger's Chemical Journal, iii. 219.

extremity

extremity was defended by a metallic cylinder) to one-sixth of its capacity of equal parts of oximuriatic and hydrogen gas. The tube was inverted in a bason, and filled to one-sixth of water deprived of air. The smallest electric spark inflamed this gas with such violence, that several tubes were reduced to powder. I repeated the experiment in a stronger tube; and, after having placed the apparatus under the receiver of an air pump, I exhausted the air, till the gas occupied nearly the whole of the tube, that is to say, a space six times greater than before. though most inflammable, are most readily deprived of that property by expansion.

The electric spark was then passed through it, and I saw its course, but it was not possible to inflame this dilated gas. After having restored the ordinary pressure of the atmosphere, a slight diminution of volume was observed, which indicated the slow formation of water. There was, nevertheless, enough of the undecomposed oximuriatic gas to be inflamed by the electric fluid. I could not exactly determine by this means the degree of dilatation at which a mixture of these two gases loses the faculty of inflaming, because the receiver was not hermetically closed. But I perfectly accomplished the intention of this experiment, namely, to prove that this very inflammable gaseous mixture loses its faculty of burning at a much less degree of dilatation than a mixture of oxigen and hydrogen gas, which does not cease to be inflammable, until dilated more than sixteen times its primitive volume.

May not the cause of this anomaly consist in the circumstance that, in the oximuriatic acid the oxigen is in a more concrete state, and, consequently, contains less caloric than oxigen gas does; so that, when at a certain dilatation, some of the parts of the oxigen of the oximuriatic gas are applied to a part of the hydrogen, the heat developed is not sufficient to effect the continuation of union in the two principles of the rest of the gas? Speculation on the cause.

IV. We have here an additional proof that the varied pressure of the atmosphere remarkably changes the effects of chemical affinity. The inflammable property of hydrogen gas, and of all the combustible bodies, would certainly be unknown at a suitably diminished pressure of the air; and we may suppose, on the contrary, that many bodies would appear to us very inflammable, at a pressure of from two to ten times as great. The Deductions.

nitrous

nitrous muriatic and sulphurated hydrogen gases might, perhaps, under these circumstances, appear as inflammable as phosphorated hydrogen gas.

Well-conducted experiments of this kind promise to afford results of great interest to science.

Rarefaction by heat has the same effect as diminished pressure.

V. Instead of rarefying the gaseous mixtures by means of the air-pump, I made use of heat.

Experiments.

The tube was filled with mercury, and inverted in a bason of the same metal. I passed up one inch of detonating gas, composed of two parts of common air, and one of pure hydrogen. By means of a moveable spirit lamp, I heated the upper part of the tube until the gas occupied four times its original volume. As the gases were not previously dried, they must have contained much moisture, which favours the dilatation; for, without the presence of water, so great a dilatation could not, perhaps, have been effected at that temperature. I then passed electric sparks from the conductor of the machine, and by leyden vials of middling size, without ever succeeding in setting fire to the dilated gas. As soon as the gaseous mixture began to cool, and the space it occupied no longer exceeded three times its original volume, it took fire by a weak spark, and it was easy to observe, that the inflammation was proportioned to the density of the gas.

By continuing to cause the electric fluid to act upon the dilated gas to a point which rendered the subsequent inflammation impossible, I had farther occasion to observe a quiet composition of water.

Flame applied instead of electricity.

VI. In order to determine how the detonating acid, in this state of dilatation, would comport itself at the approach of a lighted taper, I filled a tube to one-fourth, which I heated so as to cause all the mercury to descend, and part of the gas began to escape from the tube. At this moment I approached a lighted taper, but it did not take fire. I left the tube in the mercury, and after cooling, I closed the lower orifice with my finger, then inverted the tube, and the gas took fire with explosion, on the approach of a lighted candle.

This experiment seems to prove, that caloric cannot be considered otherwise than as an indirect cause of explosion. By

its

its expansive force it would, no doubt, oppose the union of hydrogen and oxigen, an insurmountable impediment, if the resistance of the atmosphere did not exercise a reaction which, under certain circumstances, becomes a maximum of compression.

7. All these facts, joined with those of my former Memoir, may establish the following theorem : General theorem.

Hydrogen gas in the atmosphere, under a pressure of four or five times that of the ordinary state of the lower air, is not capable of being set on fire by the electric spark, nor by a lighted candle, whether that dilatation be owing to a diminished pressure, or a more elevated temperature.

I take the extreme limit of dilatation here as it was observed in my experiments with hydrogen gas and common air. But there is no doubt that the two gases, if mixed in a proportion less favourable to combustion (as for example, two or three parts of hydrogen to one of air) would not require so great a dilatation to deprive them of the faculty of inflammation. Hence it may happen, that at the bottom of the salt mines of Cracovia, or at Amsterdam, or any other low town, it might be found, that a gaseous mixture might be inflammable, which would not be so at a more elevated town, such as Quito, in South America. This would depend on the proportion in which the hydrogen might be mixed with the atmospheric air. The limits of dilatation will vary with the proportion of component parts.

8. Various phenomena may be, without difficulty, explained from this theory, of which I shall give a few examples. Explanation of other facts from this doctrine.

A great number of combustible bodies inflame on passing in the state of oxide into the oxymuriatic gas. Much more would it, therefore, follow, that the oxymuriatic gas would be decomposed by charcoal at an elevated temperature. Nevertheless, M. Thenard and Gay Lussac have lately discovered, that oxymuriatic gas may be passed through ignited charcoal without the gas being decomposed.

Interesting and instructive as this experiment may be in itself, as to its explanation, I do not accede to the opinion of these chemists on that head. According to their reasoning, the oxymuriatic gas is decomposed only when it meets with hydrogen or water ready formed. For my part, I am persuaded, that the presence of water is an indispensable condi- Oxymur. gas does not inflame with lighted charcoal.

 tion

tion in every chemical action*. We must recollect, that water is present in the oxymuriatic as well as in every other gas; and, by the experiments of Priestley, Kirwan, Berthollet, Cruickshank, &c. it is put beyond doubt, that charcoal, heated as long as possible, contains still a quantity of hydrogen, of which the decomposition ought to follow; and, as it does not, the reason given is insufficient.

Investigation of the cause.

9. I will examine this subject rather more fully. I think the true cause of the non-decomposition lies in the great dilatation to which the oxymuriatic gas is subjected by the heated charcoal. In this case it comports itself like the hydrogen in my first experiment. At a certain degree of expansion it can no longer be decomposed. Neither do I doubt, as a consequence of what has preceded, that oxigen gas can freely pass through ignited charcoal, provided that degree of dilatation be acquired, which may be considered as the limit of combustion of this gas, whether that degree be produced by an elevated temperature, or by any other circumstance.

Difference between the effect of an ignited body and flame.

10. Another well-known phenomenon, not sufficiently explained, as prevented by hydrogen gas, which, notwithstanding its great combustibility, does not take fire by an ignited body, though it does by flame. The following observations, compared with our theory, may, perhaps, throw some light upon the subject. When a well-ignited piece of charcoal is taken up with a wire, and the finger is brought within an inch of it, the heat is scarcely to be borne; but we can easily hold the finger at half that distance from a lighted candle, without perceiving any sensible heat.

Explanation.

In this case it appears, that flame insulates the heat it derives from the burning wick, or that it is carried upwards by the flame.

* See my Memoir in the Annales de Chimie, vol. LXII, the works of Scheele, edited by Hermbstadt, p. 228; and Mrs. Fulham's Essay on Combustion, London, 1794.—G.

To the above note M. Vogel adds;

M. Ruhland, of the Academy of Munich, who is at this time at Paris, has published an interesting Memoir in the Chemical Journal of Schweigger, on the necessary presence and fixation of water during the act of combustion. He has shewn, that a great number of combustible bodies cannot burn in well dried air, and that water is very much concerned in the union of oxigen with them.

flame. I suspect that the ignited body, before it touches the hydrogen, dilates it slowly within the sphere of its activity, so as to render it incapable of taking fire, whereas the flame exerts no action till it touches the gas. At this moment the gas dilates itself suddenly, and with violence, at the part in contact, and by that means produces, in the parts adjacent, that compression which determines the combustion. This explanation is also confirmed by the following facts: I could not set fire to hydrogen gas contained in a narrow tube, which I gradually and vertically brought near the upper extremity of a lighted taper. In this case the gas is dilated in the same manner as by an ignited body.

11. Since in all these experiments the dilatation must be attributed in part to the vapour of water, which is inseparable from the gases, I was desirous of ascertaining whether other very expansible fluids, such as alcohol and ether, would change the degree of dilatation, which may be considered as the limit of the combustion of hydrogen gas. The experiments I made on this subject led to other results.

Water is of great importance in combustion. Trials with alcohol.

The tube, capped with a metallic cylinder, as before described, was filled with mercury and a drop of alcohol. I then passed to half the metallic cylinder a mixture of two parts air and one hydrogen. The drop of alcohol was, therefore, between the gases and the mercury. I then heated the tube till the alcohol began to be converted into vapour.

It was in vain that I attempted to inflame the gas dilated to four times its original volume, by the electric spark of an inch in length. I obtained the same result with the vapor of ether, and consequently the limit of inflammability depends solely on the degree of dilatation. When the tube was quite cold, the gas occupied a larger volume than before, and it was not possible to set fire to it by the strongest electric spark, though it still burned with a blue weak flame by the contact of a lighted taper. Hence I concluded, that the alcoholic vapor was decomposed; that its elements had united with the detonating air to form an oxicarburetted hydrogen gas, which is very variable in the proportions of its components. I was more confirmed in this opinion by the sudden expansion I had remarked in gas charged with alcohol by means of the electric spark.

Expansion with vapour of alcohol prevents combustion.

The alcoholic vapor decomposed by electricity.

12.

Repetition of the exp. with variations. 12. In order to ascertain more clearly the nature of the gas, I filled the tube with mercury, and then introduced a drop of alcohol. I heated the alcohol till the mercury had descended one inch below the metallic cylinder, which served to conduct the electric spark. I brought this small apparatus near the conductor of the electric machine, and passed several hundred electric sparks through it, at the same time that I was careful to keep up the dilatation by heating the tube from time to time. The electric light, under these circumstances, shewed a beautiful green colour, and formed a torrent of celadon green light, which had a beautiful effect in the dark.

Decompos. of the fluid alcohol by electricity. While the tube became cold, the alcoholic vapor was condensed into a thin stratum of liquid, which, by the rise of the mercury, approached the metallic cylinder. At the moment the stratum of alcohol touched the metallic cylinder, a decomposition of the alcohol, in the liquid state, commenced. The surface of the mercury then became covered with bubbles scarcely visible, which were from time to time inflamed by the electric sparks, and formed in the middle of the liquid small brilliant points, resembling the flame of phosphorus. It appears that alcohol is slowly decomposed in oxigen, and in inflammable gas. The alcohol in vapor is still more rapidly decomposed; but the product is an uniform gas, composed of oxigen, hydrogen, and carbon. By a quarter of an hour's electric action, I obtained half an inch of this gas, which is carbonated hydrogen, as I have proved by experiment.

I could not decompose the vapor of water in so short a time by means of the electric spark. It appears that water is more difficult of decomposition, or that its elements reunite slowly after their separation to recompose water.

Green colour of the electric spark in vapor of alcohol. 13. The beautiful celadon green, which the electric spark assumes in the vapor of alcohol, induced me to examine several other bodies in the state of gas. According to Priestley, the electric spark is of a purple red in hydrogen gas; but as it takes this tint in every other dilated gas, (as, for example, in air dilated by the air-pump,) we might, in the former case, attribute the colour to the rarity of the hydrogen. The nature of the electric fluid must nevertheless have some influence on the colour of the electric spark; for the spark appears red in ammoniacal gas, and in phosphorated hydrogen, though those fluids

fluids are much denser than hydrogen gas. It is possible that the conducting power as to electricity, which must be peculiar to each gas, may be concerned in this effect; and Priestley supposes, not, perhaps, without reason, that the red colour may be an indication that the electricity is transmitted with difficulty.

The colour probably depends on the conducting power of the gas.

In the vapor of boiling water the electric spark is of an orange yellow, and in dried carbonic gas, as well as in oxigen gas, I have always seen it of a very beautiful violet.

In steam the spark is yellow.

In order to dry these gases as much as possible, I have left them to remain over pure lime a little heated, which absorbed the water of the carbonic acid until it was saturated.

We see, therefore, in these experiments, the electric light appears with the colours of the solar spectrum red, orange, green, blue, and sometimes violet, a phenomenon which agrees with the observations of Ritter. This philosopher applied the positive pole of the pile to his eye, while he brought his hand into contact with the negative pole, and he observed that objects then appeared to him brighter and bluish; but, on the contrary, when he applied the negative pole to his eye, objects became deeper and of a reddish colour. (See Gilbert's Annals, VII, p. 447.) When the carbonates dissolved in water, or even pure water, is exposed to the current of the voltaic pile, a decomposition takes place; the carbonic acid, or the pure oxigen is separated by the positive pole, and it is precisely in these two gases that the electric spark appears blue; but it appears red in hydrogen gas, phosphorated hydrogen, ammonia, sulphurated hydrogen, and probably in all the gases which are disengaged at the negative pole*.

All the prismatic colours are seen in different sparks.

Voltaic speculations.

14. In atmospheric air, which I had compressed by a column of two feet of mercury, I caused the electric spark to pass. The spark was brighter, but not coloured. It is weaker in hydrogen and in rarified air. From these observations, I think it may be concluded, that the intensity of the electric

Electric spark brighter in condensed air.

* From some experiments, not yet sufficiently repeated, I think there is reason to conclude, that the colour of the electric spark is governed by the refracting power of the medium upon light; that at the maximum the colour is red, and at the minimum violet. Note of M. Schweigger.

light

light is always directly as the density of the gas, and inversely as its conducting power for electricity.

We might conclude, *à priori*, that the light ought to appear stronger, the greater the resistance to be overcome, and that for this reason it is brighter in carbonic acid gas and oxigen gas, when dried, which have a more considerable specific gravity than hydrogen and atmospheric air.

Polar or voltaic electricity of extensive influence throughout nature.

If we compare these observations on the colour of the electric spark in different mediums with the remarks of Ritter, and the experiments of Rochon, Herschel, and Leslie, on the power which generates heat, and with the interesting results of Scheele upon the reduction of the simple rays of the prismatic solar spectrum; if we consider, in the next place, the relation between these phenomena and those of the galvanic battery, we shall have reason to conclude, that the time has arrived when the polar or galvanic electricity ought to be recognized as the principal agent in all chemical phenomena. Analytical researches of this description are calculated to throw great light upon every department of philosophical knowledge.

VII.

An explanatory Statement of the Notions or Principles upon which the systematic Arrangement is founded, which was adopted as the Basis of an Essay on Chemical Nomenclature. By Professor BERZELIUS.

(*Continued from p.* 319, *Vol. XXXIV.*)

Stibiate of potash.

THE manner of obtaining this salt is to burn one part of antimony in powder, with six p. of nitre in a crucible, and give the greatest heat it can support without fusion. It is then to be reduced to powder, and washed with cold water as long as the water extracts either alkali or nitrate of potash. The water at last leaves a powder of an acrid and slightly metallic taste, very little soluble in cold water; but boiling water dissolves a large part of it if exposed to its action for some hours. The liquid, if filtered while still hot, throws down nothing by cooling. When evaporated to the consistence of syrup

syrup, it deposits a crust of white powder, granulated as if tending to crystallize, and very light. If the evaporation be continued to the consistence of honey, the mass congeals during the refrigeration, presenting a white semitransparent pliable crust. When evaporated to dryness, it affords a white mass like enamel, brittle, and cracked in every direction. The neutral stibias kalicus is difficultly soluble in cold water, but is perfectly soluble in boiling water, without affording any deposition during the cooling. When dried at the temperature of +100° (212° Fah.) and afterwards exposed to the fire, it looses its water of crystallization to the amount of 11½ per cent. of its weight. Five grammes of stibias kalicus, previously ignited, being treated with nitric acid, were decomposed without the smallest disengagement of carbonic acid. As it is very difficult to separate the potash completely, I digested the insoluble part with new portions of acid until no more potash was by this means extracted. By this means I obtained 2·256 grains of nitrate of potash, which contain 1·04 grs. of pure potash. The stibic acid, being heated to redness, left 3·69 grs. of white oxide, equal to 3·96 p. of acid deprived of water. The stibias kalicus is therefore composed of

Stibiate of potash.

Stibic acid	79·2	100
Potash	20·8	26

100 p. of stibic acid contain 27·3 p. of oxigen ; and 26·3 p. of potash contain 4·471 or 4·471×6=26·826. It appears, therefore, that the stibic acid combines with a quantity of basse, which contains one-sixth as much oxigen as the acid itself, because we have found the same in the combination of the acid with water. If we compute the quantity of water found in the dry stibiate of potash, it will be seen that it contains nearly three times as much oxigen as potash ; and it would become accurately so if we admit that the result is inaccurate to the precise extent in which it differs from the calculation. I must, however, observe, that in repeating the experiments on the composition of the stibiates, I have had various results, and that I cannot pretend that they are entitled to very great confidence, as far as relates to the accuracy of the numbers. The results, nevertheless, which I here communicate, have been deduced from experiments made with the greatest care ; and if I have been deceived, it must be ascribed to the great difficulty of pro-

Stibiate of potash.

producing a determinate or neutral compound when the affinities on which it depends are so feeble.

The powder obtained by edulcorating the product of nitre burned with antimony, and from which boiling water extracts the neutral stibias kalicus, appears to be a stibic acid, which is decomposed by the action of the boiling water into a neutral stibias and a stibic acid, or rather into a superstibias, with a still greater excess of acid. 100 parts of this powder, well washed with cold water (but not treated with boiling water) leaves, after treatment with nitric acid, a quantity of stibic acid combined with water, which, in one experiment, produced 83·74, and in another experiment 84·00. It follows, that 100 p. of stibic acid had been combined with 11·33 p. of pure potash. This quantity is rather less than half what is contained in the neutral stibiate. It appears that the effect of the cold water has changed it a little by dissolving a small quantity of neutral stibiate, and that, in fact, there is found a certain portion of the latter mixed with the nitrate and with the combination of oxidum stibicum and potash, which are taken up by the cold water.

This circumstance explains why, in the prescription for making the calx antimonii elata of the pharmacopeia, it is expressly forbidden to fuse the mass, because, in this case, the superstibias kalicus decomposes the nitrite of potash, by disengaging its acid, and becomes a neutral stibiate, which is almost totally dissolved during the washing.

Ammoniacal superstibiate.

The stibias hydricus is dissolved by strong digestion in caustic ammonia, and its solution appears to contain a neutral stibias ammonicus. It cannot be evaporated because, in this case, part of the ammonia would fly off, and a white precipitate would be formed, which reddens turnsole paper, and affords ammonia and water when heated in a glass retort, the stibic acid remains in the form of a yellow powder. The precipitate is, therefore, a superstibias ammonicus. This is not decomposed by the action of air, even if exposed at the temperature of 40° or 50° (105° to 120° Fah.) for the space of several months.

Barytic stibiate.

Stibias baryticus is a white powder which is obtained by precipitating the muriate of barytes with stibiate of potash. It appears to be perfectly insoluble in water, because the precipitate produced by the first drop of the precipitant is not redissolved

dissolved when shaken in the solution. This salt is flocculent, light, and does not adhere to the sides of the vessel. It is not decomposed by carbonic acid in the air; but the nitric acid extracts by digestion, though not without difficulty, all the barytes it contains.

Stibias kalicus is produced by a process similar to the foregoing. It is a white powder sparingly soluble in water; for the precipitate produced at the commencement is readily dissolved by shaking the solution. Yet it requires but a small quantity to saturate the water, and the precipitate, after a time, assumes a crystalline aspect precisely like the carbonate of lime, and a quantity of these infinitely small crystals is deposited on the glass, which becomes covered with them. It is found, however, that it is not carbonate of lime by decanting the fluid, and treating it with nitric acid, which dissolves the lime without effervescence, and leaves the stibic acid on the glass in the form of a milk white stratum. Stibias kalicus.

Stibias plumbicus is produced by precipitating nitrate of lead by stibiate of potash. A white precipitate is formed, resembling very much at first the muriate of silver, and which is perfectly insoluble in water. When exposed to the fire it gives out its water of crystallization, and becomes yellow. It does not melt by a red heat, but on a piece of charcoal before the inner flame of the blow-pipe it is rapidly reduced with a slight deflagration, affording a white metallic button of *stibiteum plumbi.* I was in hopes, by the analysis of this combination, of gaining a more exact knowledge of the composition of the stibiates, as it was very easy to obtain the stibiate of lead in a neutral state. But I found, to my great surprise, that the stibiate of lead, ignited by heat, was scarcely at all attracted by the nitric acid, and that the newly-precipitated stibiate still humid, if poured into concentrated nitric acid, cannot be decomposed but to a certain degree, which seems to produce the superstibias plumbicus. I endeavoured to decompose this by several days exposure to concentrated nitric acid, nearly boiling, without its being altered. I lastly mixed it with charcoal, and reduced it in a glass retort, and afterwards treated the reduced metal with nitric acid. The result was nitrate of lead and superstibias plumbi again; and I could not, Stibiate of lead.

not, consequently, obtain a more precise result than I have before stated.

Stibiate of zinc.

Stibias zincicus produces a heavy precipitate, soluble to a certain degree in water, which is at last deposited in the form of a powder, which covers the sides as well as the bottom of the glass, and, after washing and drying, has a crystalline appearance like carbonate of lime. When heated in a glass retort the stibiate of zinc gives out its water of crystallization, and becomes yellow. It is not reduced by the blow-pipe upon charcoal.

Stibiate of manganese.

Stibias manganosus is a white powder, very little soluble in water, and preserving its colour in the air as well when humid as when dry. In the fire it gives out water and becomes greyish, which seems to be owing to a slight oxidation. Exposed to a very elevated temperature, the stibias manganosus resumes its white colour by a change, concerning which I shall speak hereafter.

Stibiate of iron.

Stibias ferrosus is precipitated perfectly white, and preserves its white colour as long as it is covered with water. If it be put on the filtre and washed, it changes its colour, by the contact of the air, into a yellowish grey. By heat it gives out water and becomes red. Upon charcoal in the interior flame of the blow-pipe, it becomes reduced. The antimony is volatilized, and the iron remains obedient to the magnet.

Stibiate of cobalt.

Stibias cobalticus is the most soluble of the stibiates, though its solubility is not very considerable. The precipitate which is first formed is again dissolved, until a certain quantity of the precipitating liquor has been added. The precipitate is red flocculent, and contracts, after a time forming crystalline grains of a rose colour, which are deposited mostly on the sides of the vessel. When exposed to a cherry red, it loses its water of crystallization, and is diminished precisely one-third of its weight. The stibiate deprived of water is of a deep violet blue. By heat in an open vessel it becomes of a blackish brown, probably by the increased oxidation of the oxide of cobalt. If what I have stated of the capacity for saturation of the stibic acid be well founded, this salt will be composed of 52 p. acid, 14 oxide of cobalt, and 33·3 water. The oxide of cobalt contains 2·99 p. oxigen and 29·39

water.

water. That is to say, scarcely different from ten times as much.

Stibias cupricus forms a green precipitate very voluminous and perfectly insoluble, which, after drying, has a very pale verdigris colour. When exposed to fire, it loses about 19 per cent. water of crystallization, and becomes green. Heated before the blow-pipe upon charcoal it is reduced with a lively deflagration, and produces stibium cupri in form of a metallic globule of a very pale copper colour. Stibiate of copper.

In a solution of corrosive sublimate the stibiate of potash at first affords no precipitate; but after some time the mixture deposits a grey yellowish very light mass, which, for the most part, remains suspended in the liquor. The filtered solution does not contain the smallest share of stibic acid.

Will it be necessary, in this place, for me to remark, that the compounded bodies, of which I have here given a superficial description, were, in fact, chemical combinations, and not precipitates, formed at the same time and mechanically mixed together? Their crystalline appearance, their solubility in water, as well as the circumstance, that the stibiate of magnesia preserves its colour in the air, which could not have happened if the compound had been a mere mixture of the oxides of antimony and of manganese, prove that these substances were actually substances analogous to salts of sparing solubility in water. These compounds are not mere mixtures, but ch. combinations.

The stibic acid, in common with other weak acids, has the property of combining with stronger acids, to which it performs the functions of a base in the same manner as the boracic acid does with the sulphuric and the fluoric, and as the carbonic* and the arsenious acids do with the muriatic. The stibic acid Stibic acid as a base, &c.

* Mr. John Davy, a distinguished English chemist, has discovered a combination of muriatic and carbonic acids. He produced it by passing electric discharges through a mixture of oximuriatic gas and gaseous oxide of carbon. The two gases combined in equal volumes, and did not then occupy more than half the space they occupied before. The excess of oxigen in the oximuriatic gas is precisely the quantity requisite to convert an equal volume of gaseous oxide of carbon into carbonic acid; and, according to the calculation of the composition of the muriatic acid, it is found that, in this new combination, the muriatic acid and the carbonic acid contain an equal quantity of oxigen. B.

is

is slightly dissolved in the concentrated muriatic acid. The solution is yellow, and affords a precipitate by the addition of water, which is a triple combination of the muriatic and stibic acids and water. If this be briskly heated, there is a disengagement of oximuriatic gas, (superoxidum muriatosum) and a white oxide remains. If, on the contrary, it be heated slowly, muriatic acid is disengaged, and the acid remains of a yellow colour. In some instances it is obtained partly white and partly yellow.

6. *Combinations of the stibious acid with saline bases. Stibiites.*

Stibiites stibious acid. I fused together the white oxide of antimony and caustic potash; the former in excess. The fused mass treated with cold water, produced a weak alkaline solution, and left a white powder undissolved. This powder exposed for two hours to the action of boiling water was in part dissolved, and the solution greatly resembled that of the stibiate of potash. The acetic acid, as did also a current of carbonic acid gas, produced a white precipitate, which re-acted like an acid upon vegetable colours, and when heated over a spirit lamp afforded water, and left a white oxide which inclined a little to yellow while still hot. I am disposed to think, that this last circumstance was owing to the formation of a small quantity of stibic acid, from that action of the alkali and the heat, because the pure white oxide does not become yellow by heat. I, therefore, consider this precipitate as the combination of an acid with water, and as this acid has antimonium for its base, though it contains less oxigen than that which I have already described, I shall call it *acidum stibiosum*, and the combination with water, *stibiis hydricus*.

Stibiite of potash, A solution of the neutral stibiite of potash being decomposed by nitric acid, produced 4·6 grammes of stibious acid ignited by the fire, and 3·16 gr. of nitrate of potash well dried. These are equal to 1·4 gr. of pure potash, of which the oxigen is 0·2482. The oxigen of 4·7 gr. of the stibious acid is 1·025 and $0{\cdot}2482 \times 4 = 0{\cdot}9928$; whence it appears that the stibious acid, has the capacity of neutralizing a quantity of base of which the oxigen is one-fourth that of the acid.

The stibiis kalicus* must therefore be composed of

Stibious acid - -	76·6 - -	100·00
Potash - - - -	23·4 - -	30·55

* The calx antimonii elata is, in fact, a superstibiis kalicus.

The

The *stibiis baryticus* is slightly soluble in water and does not change by the action of the air. If a boiling solution of stibiite of potash be let fallen drop by drop into a boiling solution of muriate of barites, the stibiite of barites crystallizes gradually on the tube, in the form of small white needles of a silky brilliancy. of barites,

The *stibiis kalicus* forms a crystalline powder, which is white and very little soluble in water.

The *stibiis plumbicus* perfectly resembles a stibiiate of the same base. The *stibiis manganosus*, ferrosus and *zincicus* likewise resembles the stibiate, with the same bases, but they are rather more soluble in water. of lead,

The stibiis cobalticus is considerably soluble in water. It precipitates in the form of a voluminous lilac powder, inclined to red, and does not assume the smallest appearance of crystallization by drying. When heated, it loses one fourth of its weight in water of crystallization, and becomes greenish grey. If a neutral solution of cobalt be precipitated by a stibiate of potash, which contains potash in excess, a violet precipitate is formed, which is still more soluble than the former, and appears to be a *substibiis cobalticus*. of cobalt,

Stibiis cupricus is a powder of a very pale greenish blue, perfectly insoluble in water. When heated it loses its water of combination and becomes green. A stibiite of potash, with excess of the latter precipitates the salts of copper of an apple green, resembling the arsenite of copper (Scheele's green.) In the fire it becomes black, and when pulverized it assumes a yellowish grey. It appears to be a substibiis cupricus. of copper.

We have seen, therefore, that antimony possesses the same property as arsenic, molybdena, sulphur, phosphorus, and azote to produce different degrees of oxidation in its acid, and that the saline combinations formed by these two acids greatly resemble each other, in the like manner as it has been long proved in chemistry, that there are arseniates and arsenites. Antimony also has the analogy with sulphur, of producing a salifiable base, which combines with the muriatic acid, but which is easily separated by water, with which the acid combines in preference, as it is a more powerful base than those oxides. Thus antimony has two degrees of acidification and can act as a base.

8. *A particular*

8. *A particular phenomenon produced by some of the stibiates and stibiites.*

Remarkable combustion of some of the compounds.

Before we quit the oxides of this metal, I shall mention a singular phenomenon which several of the stibiates and stibiites possess; and of which the explanation throws very great light upon the theory of chemistry.

If in a small crucible, or spoon of platina, the stibiate or stibiite of cobalt, copper, or zinc, first deprived of their water of combination be heated before the blow pipe at a certain temperature, it takes fire and burns for a few moments with an extremely vivid ignition. After cooling, the mass is found to have undergone a very remarkable change. The stibiate of cobalt becomes of a pale red, or rather a brick colour; and the stibiates of copper and of zinc are both white.

As the two metals which are in these salts, have been carried to the highest degree of oxidation, this phenomenon cannot be explained by an ulterior oxidation. The stibiates which, before this operation, were very easily decomposed by stronger acids, now resist their action. The most concentrated muriatic acid, no longer decomposes them, and if it dissolves a small quantity, it takes up the acid and the base alike. Nevertheless, after continued digestion for several weeks, it appeared to me, that these substances had undergone a partial decomposition, though not to be compared with that, which, before the ignition would have taken place at the first instant of contact with the acid. The concentrated muriatic acid, does not develope the slightest trace of oxymuriatic gas, which proves that the metals are not in an higher degree of oxidation. I have not observed any similar effect in heating the stibiates of potash or of the earths, neither has this ignition appeared in the stibiates of lead and manganese, though by the action of fire they became insoluble in acids, and the stibiate of manganese resumed its ordinary white colour.

If either antimony in powder, or the stibious acid be mixed with the red oxide of mercury, and exposed to the fire, the antimony produces a detonation accompanied with vivid ignition, metallic mercury rises in vapour, and there remains a very voluminous powder of a deep olive colour. I at first supposed that this powder, which had undergone strong ignition, and consequently

consequently should not contain mercury, was a superoxidum stibicum. I endeavoured to decompose it in a small retort to disengage the excess of oxigen, but to my great surprize I found mercury condensed in the neck of the retort, while the colour of the powder became changed to yellow. I endeavoured, therefore, to decompose this combination, by means of concentrated muriatic acid, with which I kept it in digestion. The acid in fact dissolved a small quantity, but, on diluting the solution with water, the greatest part was deposited. The precipitate thus formed was greenish grey, and appeared to contain less oxide of mercury than before. The portion not dissolved by the muriatic acid, still preserved its colour and all its properties. I separated the acid from it, and washed it very well, and afterwards dried it by heat approaching to ignition. 200 parts of this dried combination exposed to a moderate heat in a glass retort exactly weighed, until nothing remained in the retort, but the yellow stibic acid lost by this evaporation 31·5 p. in weight, of which I found 29 p. of metallic mercury in the neck of the retort. The receiver contained oxigen gas; and therefore, the loss of 2·5 p. must be attributed to this gas, and corresponds very nearly to the oxigen required to convert these 29 parts of mercury into red oxide (the exact quantity needful being 2·3 p.) The stibic acid which remained in the retort, lost in a still stronger heat 6·5 per cent. and became converted into stibious acid. This experiment appears, therefore, to prove, that olive coloured substance contains nothing but the stibic acid, and the red oxide of mercury; and it appears that the stibiate of mercury had undergone, at the moment of its formation, the same change as the other metallic stibiates I have mentioned do by ignition.

(To be continued.)

Extract

VIII.

Chemical Examination of a Variety of Fossil Natron, or Native Carbonate of Soda, lately discovered in South America. By AMERICO-CABRAL DE MELLO.

Fossil natron

THE native carbonate of soda, of which I subjoin an analysis, has lately been discovered in the vicinity of Buenos Ayres, and considerable quantities of it have been imported into this country.

found in strata

It occurs in compact, solid, stratified masses of from two to six inches thickness, deposited upon a bed of clay, strongly impregnated with common salt.

not crystalline.

It exhibits no vestiges of crystalline structure. Its masses are granulous, readily broken between the fingers, and of a light yellowish grey colour.

It is readily soluble in water, and scarcely leaves any insoluble residue.

Exp. 1. For carbonic acid,

Experiment 1. A solution of the Brazilian fossil natron, containing of 100 grains of salt, after having been neutralized by nitric acid, lost 44·25 grains.

and sulphate of soda,

2. In this solution nitrate of barytes was dropped till no farther cloudiness ensued. The obtained sulphate of barytes weighed four grains, which are equal to 1·25 grains of sulphate of soda.

and muriate of soda,

3. To this fluid, from which the sulphate of barytes had been separated, nitrate of silver was added, till it occasioned no farther change in the solution. The produced muriate of silver, after having been dried at a heat short of its melting point, weighed twenty grains, indicating 9·25 grains muriate of soda.

and water of crystallization.

4. To learn the quantity of water of crystallization contained in this salt, 100 grains of it were ignited in a platina crucible for one hour. The mass, which was of a porcellaneous appearance when cold, was dissolved in a weighed quantity of nitric acid to ensure the complete expulsion of the carbonic acid contained in the salt; and from the loss of weight which the 100 grains of fossil natron had suffered, after having deducted

ducted the quantity of carbonic acid belonging to it, as determined in experiment 1, its proportion of water of crystallization was found to be 20·75 grains.

From this statement it therefore becomes evident, that the composition of 100 parts of fossil natron is as follows: Summary of component parts.

Carbonic acid	44·25
Soda	24·25
Water of crystallization ..	20·75
Sulphate of soda	1·25
Muriate of soda	9·50
	100·00

Muriate of platina, and several other tests, indicated the absence of all foreign bodies in the solutions, expts. 1 and 2, which, after being evaporated, yielded pure nitrate of soda.

This variety of natron, when exposed to the atmosphere, does not, like the common sub-carbonate of soda, obtained by artificial processes, effloresce, a circumstance which unquestionably must be attributed to the comparatively small portion of water of crystallization which it contains, and to the complete saturation of its alkaline base with carbonic acid. It does not effloresce.

5, Nassau Street, Soho,
April 13*th*, 1813.

IX.

On the Sensation and Perception of Plants.

To Mr. Nicholson.

SIR,

I AM induced to offer a few remarks on the undecided question of the existence or non-existence of vegetable sensation and perception, though I cannot, for my own part, even give an opinion on a subject in which so much discussion, and so many opposing arguments and facts are involved.

I confess I think, that the circumstances of the tendrils bending towards neighbouring supports, and their twining round Motion or flexure of tendrils well accounted for by Mr. Knight without admitting sensation.

round slender bodies within their reach, on which so much stress has been laid by the advocates of the existence of perception and sensation in plants, have been more than plausibly at least, if not satisfactorily, accounted for by Mr. Knight, in his excellent paper on the subject, in the Philosophical Transactions for 1812*, in which he attributes these effects, first to the action of light on that part of the tendril which is exposed to it; and, secondly, to the pressure which is made on one side by the substance round which it twines, and the consequent expansion of the opposite part.

Apparent irritability of lettuce.

The excessive irritability† (as it has been, I think, erroneously called) of many plants, the lettuce, for instance, has also been often adduced in proof of the sensitive power of vegetables, though I must say I think without any reasonable foundation. I beg leave to submit, whether this supposed effect of irritability may not arise from the following cause: During the period of flowering, the vessels are so distended with the proper juice of the plant, that a considerable pressure must take place on all sides; so that when the plant is wounded, even in the slightest degree, the fluid finds an immediate, and frequently a forcible, egress. When the distention (if I may use the term) is very great, the juice sometimes bursts out spontaneously, and without any previous wound whatever.

Can the motion of mimosa s. and other very sensitive plants be explained without sensation?

I merely submit this for the consideration of those who have greater opportunities for observation, and are more intimately acquainted with vegetable physiology. Yet allow me to ask your correspondents one question—How can we, but by allowing the existence of vegetable sensation, account for the powerful motion of *mimosa sensitiva*, m. niloctica, and many other extremely sensitive plants; and, indeed, numberless other phenomena which I should be very glad to see once more fairly and ably discussed?

I am, Sir,

Your obedient Servant,

Poole, April 6th, 1813. T. B.

* Vide Journal, vol. XXXIV, p. 42.

† Vide Journal, vol. XXXII, p. 138, for M. Corradori's experiments on the *sonchus asper*, &c.

On

X.

On the Preparation of Forest Trees for immediate Use, and increasing the Duration of Timber. Communicated by Capt. LAYMAN, *of the Navy.* (From his " PRECURSOR.")

On the durability and melioration of timber for naval construction.

THE juices of a tree being, like the blood of an animal, essential to vitality, but tending to corruption immediately after dissolution, accounts for the well-known fact, that the duration of timber is in proportion to the quantity and nature of the juices contained therein at the time of felling and when brought to use. It is, therefore, obvious, that by withdrawing such juices or blood from a tree while standing, the oak (as expressed by the celebrated Roman architect Vitruvius, and by Pliny,) " will acquire a sort of eternity in its duration." But as neither the mode mentioned by those celebrated ancients of cutting a kerf round the bottom of a tree while standing, as performed in Bengal, or the one suggested by Dr. Plott, of decorticating the tree, leaving it standing, as practised by the natives of Malabar for ages, will effect this desideratum, I made the following analysis :

On the 1st of June, 1812, I made experiments upon growing young oaks, one of which, that had been operated* upon, was converted the next day, increased in strength in the proportion of 436 to 609, and when doubly prepared, to 846. And, as a test of comparative duration, I made extracts from the heart and sap of the same tree in its natural state, and when prepared. The following is the result :

1. The sap, or embryo wood, in its natural state, speedily concreted, and mucor or mould was formed in fourteen days.

2. The heart, in its natural state, contained much less putrescent matter than the embryo, but a larger portion of gallic acid, and acrimonious liquid. This extract had a smell like fetid ditch water, and mucor was formed in forty-nine days.

3. The heart of the prepared oak is perfeetly sweet to the smell, and has no other appearance but a pellicle from the glutinous matter contained in the wood.

4, The embryo wood of the prepared part has the same appearance at the heart, being equally free from any symptoms of putrefaction.

* This oak, from the wetness of the season, contained 6-10ths of its weight in fluid: but in general I have found 6-11ths in June, and 4-11ths in January, to be the quantity of fluid contained in growing oaks.

On the durability and melioration of timber for naval constructions.

On the next day I proceeded to verify the facts before a well-attended Board of Agriculture, consisting of several members of both Houses of Parliament, who expressed a lively interest on the occasion: the following is the substance of a minute made by the president at the time.

Board of Agriculture, June 2, 1812.

The Board adjourned to examine some experiments made by Captain Layman, on the preparation of forest trees for immediate use on being felled, by which the specific gravity is diminished, and the sap (or embryo) wood rendered useful, as well as the strength and duration of the timber considerably increased. The following is the result, from pieces one foot in length and one inch square.

1. Poplar (Lombardy) cut from a tree in a growing state, broke with 336lb.

2. Poplar (Lombardy) counterpart piece of ditto, *prepared*, in three hours bore 368lb*.

3. Seasoned English oak broke with 784lb.

4. Seasoned English oak, *prepared*, bore 902lb.

"This piece, when broken, proved to be naturally defective internally; but a sound piece, prepared by Captain Layman, appeared to have sustained 1007lb."

5. Sap or embryo wood of oak, *prepared* and preserved, bore 930lb.

6. Counter part piece of ditto, in its natural state, broke with 536lb.

7. Common white deal, in its natural state, broke with 339lb.

8. Counter part piece, *prepared* and *preserved*, bore 508lb.

Note. Specimens were produced by Captain Layman to the Board, of the matter producing the decomposition of wood.

On the 23d of July following, I made experiments at the Navy office.

No. 1. Dry rot timber (Canada oak) of the Queen Charlotte, as received from the Navy Board, July 18, 1812.

* This experiment was made to show in how short a time wood could be prepared for use from a growing tree; but a young standing Weymouth pine, which was experimented upon with a view to masting timber, and which was three days in preparing, had not only its weight reduced, but its strength increased from 243 to 450.

2. Ditto,

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2. Ditto, ditto cured*.

3. Dry rot, and sound timber (English and Canada oak and pitch pine) of ditto in its common state.

4. Counter part pieces of ditto, *preserved*.—The above were put into bottles, and sealed up by the Navy Board; one of which was afterwards cracked, and the wood put into another bottle, which, with its counterpart, was resealed as at present.

5. A piece of English oak, broke with 228lb.—This was said to be a bad specimen, but it was a counterpart of what the Queen Charlotte was framed with.

6. A piece of sound English oak *prepared* and *preserved* bore 810lb.

7. A piece of Canada oak; of the Queen Charlotte, in its natural state, broke with 528lb.

8. A piece of ditto, *prepared* and *preserved*, bore 660lb.

9. A piece of pitch pine, in its natural state, broke with 672lb.

10. A piece of ditto *prepared* and *preserved*, bore 834lb.

As from V. to X. were broken by a lever of 5 powers, 23lb. should have been added to each. As the weight of the scales, &c. was = to $5\frac{1}{2}$.

These pieces were sealed up at the Navy-office, and remain in that state.

Upon this principle, increased duration and strength was given to teak, sissoo, and saul; which would be a great acquisition to the auxiliary aid required for our Navy. But the most important result is, that trees of our own growth that succeed on the poorest soils in Great Britain, which will not produce corn, are rendered very superior to any foreign oak imported, and preferable to the best English oak in common use for hull timber; and although some species are *naturally weaker and heavier* than foreign spars, they may be so prepared as to admit of being made into masts, yards, &c. *smaller, lighter, stronger*, and *infinitely more lasting*, than those made of American or even Russian fir.

Thus furnishing the means *to construct durable ships of British materials from the keel to the truck.*

* I must here repeat what I observed at the Navy Board at the time, "that prevention is better than cure."

In

In elucidation of the facts already stated, the following comparative view is given:

	TIMBER. Per Cubit Foot.	SPECIFIC GRAVITY. Season. lb.	oz.	Prepared Single. lb.	oz.	Prepared Double lb.	oz.	PROPORTIONATE STRENGTH. Seas.	Prepared. Single	Prepared. Double	Duration.
British Growth.	Oak, Sylvestris	54	15	43	5	39	11	790	1007	1070	Doubled.
	Oak, Do. 100 years in use							676			
	Oak, Urbana	47	4	40	8			678	822		
	Ash, Red	50	12			48	6	994		1565	
	Ash, White	42	13					790			
	Chesnut (sweet), 100 years in use	54	13					676			
	Maple (Norway)	49	11	40	11	37	0	538		1029	
	Larch (Scotch)	31	0					466	728		Increased.
PROVINCES. British India.	Teak, Malabar†	43	0	39	15			730	933		Ditto.
	Teak, Java	43	8					790	933		
	Sissoo of Bengal	55	9½	51	6½			720			
	Saul of ditto	60	2	54	4	48	7	730	950		
West Indies.	Mahogany (Spanish)	47	3					677	790		
	Lance wood	64	14					1372			
American Canada.	Oak, Red	47	0	40	13				676		
	Oak, White							528	660		
	Pine (pitch)	55	6					732	860		
Foreign.	Fir	25	15½					493			
	Teak (of Pegu)	38	11½	37	5			733	874		
	Oak (Baltic)	42	1	39	15			634	704		
	Fir (Russia)	28	11					556			
	Deal, yellow	29	9					471	622	695	Doubled.

And

* The roof of Westminster-hall, built in 1399, is formed of sweet chesnut, which probably grew in the neighbourhood, as the site of London was formerly a chesnut grove of spontaneous growth; yet the use of this fast-growing timber, which succeeds in the most barren sands, *is unknown in our dock yards*, as is that of the ash, for the purpose of ship-building, although its utility is so well known for agricultural and other purposes; and as it contains much less gallic acid than the oak, or even the chesnut, it would be less destructive to iron, and being so decidedly superior in strength to any other native wood, together with its length and clearness of ligneous fibre, is not only peculiarly applicable for plank, but superiorily adapted for thick stuff in the curve of the whale round the bows and buttocks of a ship, as may be seen in the rim of a wheel which is a whole circle made out of *one* piece. It is also admirably calculated, if properly prepared, for lower masts, which would be infinitely more lasting, at much less expence, than those we precariously procure from foreign countries.

But as this proposition may appear extraordinary to some people, it may require some little explanation. The specific gravity of Russian fir masts, is to the best English ash in their natural state in the proportion of 448 to 812, therefore an ashen mast, or a piece of timber for any other purpose made of that wood, to be of equal strength would be more ponderous; but as shewn by the result of experiments before given, the specific gravity of the

And as the sap or embryo wood would not only be rendered useful, but the timber fit for immediate conversion, it would furnish the means to do away the waste of timber and loss of time and money that take place in what is termed *seasoning*, particularly in his Majesty's dock-yards,* either when a ship is framed,

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the ash can be reduced to that of the Russian fir as 774 to 548, and the strength of the ash increasing in the ratio to the Russian fir as 1365 to 656, it follows that a prepared mast of the best English ash of only half the size would be stronger and lighter than one made of Russian fir, and the advantages over American spars would be still greater.

* As the Norway maple admits of such great increase in strength and duration, and will succeed in exposed marine situations where no other trees will, it deserves particular attention.

‡ British larch, although a tree of rapid growth upon a sterile soil, is, from a large portion of parenchyma substance, weaker and heavier in its natural state than the Riga or even American fir; yet it may be rendered superior for spars, or any other purpose for which foreign deal is imported‖.

§ Malabar teak used in ship-building may be said to undergo a kind of preparation, as at Beypour I found the weight of a very fine young teak, when cut down *with the bark on*, to be equal to 61*lbs. per* cubic foot; when the timber of similar trees decorticated for two, three, and sometimes four years before felled, was only 44*lbs. per* cubic foot, which operation is performed by the natives, in order to reduce the specific gravity for the purpose of more readily floating the trees down the river, but it also diminishes the strength, although it increases the duration.

‖ By papers presented to Parliament it appears that the expenditure of foreign spars for masts, &c. for the Navy, from the 15th of May, 1804 to the 15th May, 1805, was £7,069; since which period the consumption is increased: and the sad instance of the Guerriere's mainmast going by the board, *without being struck by a single shot*, and to which the unfortunate capture of that fine frigate by the American frigate Constitution* was attributed, is a proof that the quality of masts and (I can speak with deep regret) yards,† is not improved any more than hull timber.

* For the comparative force of English and American frigates, &c. see the Precursor, &c. just published.

† The main-yard of the Raven broke in the slings, or the vessel would not have been wrecked.

* I have tried eighteen different methods of preparing and seasoning timber, and with only one exception, found the mode or rather the custom

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framed, or, what is yet more erroneous, by placing the timber in piles, as there practised. For as not only the cause of decay, but of shrinking and rents, would be removed, it is obvious that the timber for building a ship, or for any other purpose, might be readily formed on the spot where produced, exclusive of the saving in carriage or freight of at least one-half. The timber, although converted in different parts of this country or the world, would be ready to form part of a ship the instant it was delivered into the arsenal of construction; and as the decomposition of timber commences from the moment a tree is cut down, a ship so built in six months, in a dock or slip, *under cover*, would be much more lasting than one six years in building*. And if the plank, after being prepared, was brought to and combined to the timbers *without being transversely perforated*, it is clear, that if the timber was properly moulded, the

custom in use in his Majesty's dock-yards to be the worst.—In 1805, the late Mr. Alexander Mackonochie proposed a "scheme for the ready seasoning of timber, in depriving it of its oxygen by the means of condensed steam, which would leave a vacuum, and thereby draw out the fluids from the wood, that when so freed if plunged into oil, their re-entrance would for ever be effectually precluded, and the strength of the wood found to be much increased, as well as the timber not only immediately seasoned but preserved in all its pristine state."—This appears very specious, but had the ingenious theorist brought it to the test, it would have been found to promote a tendency directly opposite to what was proposed.—Some months ago, the principle of impregnating timber was again renewed, as in a work of considerable eminence, published in September, 1812, it is stated:—"Experiments, we understand, are now making at Woolwich, on the speedy seasoning of timber, by stowing some hundred loads in a close kiln, and introducing, by means of a retort filled with sawdust, an oleaginous substance. The idea is ingenious, but we augur no useful results from the experiments themselves."—The unfortunate result a short time after is well known; for although, owing to a particular circumstance, an active ingenious person was employed, an explosion took place, by which, exclusive of the damage, several men were killed and wounded.

* The Lively frigate was 5 years in building. The Queen Charlotte, 100 guns, 7 years. The Impregnable, 90, and Revenge, 74, 9 years. The Caledonia, 120, 12 years. The Hibernia, 120, 14 years. The Ocean, 98, 15 years.

fabric

fabric would be much stronger with at least one-fourth less wood; and not only would the building of the ship be much facilitated, but in the event of requiring to shift either timbers or plank, from accident, it might be done as simply as shifting the stave of a cask. And if ships so constructed, when not wanted for active service, had the masts taken out, and were placed in a covered *dry* dock, and kept well aired by opening a plank or two on each side the bottom, the duration would be infinitely increased.

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The benefits that would arise by bringing such resources into action, and rendering ships of war more lasting, thereby reducing the consumption of timber and all other materials, with the saving in workmanship, require no comment. The great political object would be obtained of having ships at all times ready for service, when those constructed of perishable materials were rebuilding or repairing. For if the duration were in future doubled, it is evident that not only half the number of ships would be required, which might all be constructed in a Royal *building* yard, and half the expence of building and repairs on ships would be saved, and all other fabricks in which timber is used, the amount of which by the Navy Estimates for 1813 is 3,667,000l. And as the consumption of timber would also be reduced one half, only half the quantity of land would suffice to support our Navy.

XI.

Extract from a Memoir upon the Deliquescence of Bodies, by M. Gay Lussac*.

ON the 17th of May last, I communicated to the Society of Arcueil, some observations on the property of bodies to attract the moisture of the air, which chemists distinguish particularly by the word deliquescence. This property, though it still requires examination, may be referred to general principles, according to which it can easily be determined what are the bodies which possess it, the variations to which it is subject,

The deliquescence of bodies not yet regularly enquired into.

* Annales de Chimie, LXXXII, 171.

according

according to the temperature and the degree of the hygrometer at which it begins to be apparent.

Whether a body be deliquescent, is shewn by placing it in air perfectly humid.

The deliquescence of a body being dependant on its affinity for water, and the effect of this affinity being to diminish to a certain point, the elastic force of the vapour, contained in a determinate volume of air, it becomes very essential, not only for ascertaining whether the deliquescence can be shewn, but likewise to obtain comparable results, to place each body in an atmosphere completely saturated with humidity. By this means it is shewn that muriate of soda, sugar, &c. are very deliquescent; that nitre itself and many other bodies, in which this property has not been supposed, do possess it in a greater or less degree.

The degree of deliquescence

This method does not determine the degree to which a body may be deliquescent; but to ascertain this, we must take notice, that since the deliquescence of a body depends on its affinity for water, and that affinity is itself singularly modified by heat, it will be necessary to consider each temperature in particular.

is determined by the point at which a saturated solution of the body boils. If higher than 212° the body is deliquescent and more so the higher;

Suppose, therefore, that it were required to determine the degree of deliquescence of any solid, or liquid body, in air saturated with moisture at the temperature of 15 degrees centigrade (59 F.) If it be solid, the body must first be dissolved to saturation, and the solution afterwards boiled. If it boil at 100 degrees (212 F.) which is the boiling point of pure water, the body will not be deliquescent; but if it boil at a higher degree the body will be more deliquescent in proportion as the boiling point is above 100*. Thus muriate of soda will be very deliquescent in air saturated with humidity; for its solution in water at 15° does not boil, till it has arrived at 107°,4 (225 F.) Nitre will be likewise deliquescent, but much less than salt, for its solution at 15° boils at 101°,4 (214,5 F.)

The boiling points do not give the degrees of deliquescence quite strictly.

* I must here remark, that instead of taking the boiling point of each liquid, it would be more exact to take that of the tension of its vapour, at the precise temperature at which the deliquescence is required to be determined, because the elevation of the boiling point is not proportional to the quantity of salt held in solution. A similar method must necessarily be used to determine the force with which solid bodies attract the vapour of water, without any change of state succeeding, as would happen with regard to lime and the salts deprived of water of crystallization. I enter fully into this subject in the Memoir.

Experiment

Experiment here perfectly agrees with theory ; but in order to shew manifestly the deliquescence in such other bodies as are also weakly deliquescent, a small separate portion of each must be taken. These will be completely melted, whereas larger crystals would simply become moist, or liquify very slowly.

The temperature is of great consequence.

It is easy to conceive that it must be important to attend to the temperature ; for as heat greatly favours the combination of salts with water, the boiling point of each solution must vary according to the temperature at which that solution was made. Thus nitre, which is weakly deliquescent at 15° and of which the saturated solution boils at 101°,4 would be very deliquescent at the temperature of 100°, because its saturated solution made at that temperature would not boil short of 110° or 112°.

Salts not deliquescent.

The acetate of lead, and corrosive sublimate, do not sensibly raise the degree of the boiling point, and accordingly they are not at all deliquescent.

Remarkable fact : that the boiling point of water is different in glass and metallic vessels.

In determining the degree of ebullition of saline or acid liquors, I have observed a very singular phenomenon which deserves to be known. It consists in the fact, that water and other liquids boil later in a glass vessel than in a metallic vessel, unless the filings of iron, copper, or some other metal, or charcoal powder, or pounded glass be mixed with it. The difference of temperature with regard to water, is as much as 1°,3 (nearly 2° F.) and sometimes even more. This fact is of great importance in the graduation of thermometers, between which such a difference might exist in instruments made with the same care, provided the upper point of the one were taken in a glass vessel, and that of the other in a metallic vessel. It is true that if attention be paid, not to plunge the ball of the thermometers in water the difference will be less*.

No salt depresses the boiling point of water.

I have likewise ascertained that there is no salt which possesses the property of lowering the boiling point of water, though Mr. Achard has asserted the contrary.

The hygrometerical point of deliquescence determinable by inclosing the instrument under a glass wetted with the solution.

When we have ascertained the degree of ebullition peculiar to each saline solution, by means of which we acquire a

* The method prescribed by the Committee of the Royal Society, that the boiling point should be taken in steam, would remove this *probably apparent* irregularity. N.

measure

measure of the deliquescence of the salt and its affinity for water, we may proceed farther and determine at what degree of the hygrometer the deliquescence begins to take place. For this purpose it will be sufficient to place the hygrometer under a glass moistened with the saline solution, and observe the degree indicated after some hours. In this manner it will be found, that with a saturated solution of muriat of soda at 15° temperature, the hygrometer will stand at 90°, and that with a solution of nitre also made at 15° it will stand at about 97°, &c.

Instance in common salt.

Hence we may conclude, that muriate of soda will not be deliquescent below 90° of the hygrometer; but that it will begin to be so at that term, and will become much more so above it. When a table shall have been constructed, indicating the degrees of the hygrometer, corresponding with the temperature of ebullition of a certain number of salts, we may determine the degree of the hygrometer at which all the others will begin to be deliquescent, provided we know the degree of ebullition of their solutions in water.

Sulphuric acid.

I need not observe, that what relates to the deliquescent salts, is likewise applicable to all the solid or liquid bodies which have an affinity for water. According to these principles we shall find, that concentrated sulphuric acid can take from air completely humid, more than fifteen times its weight of water. And from this property, which different saline solutions possess, of exhibiting different intensities at the same temperature, it is easy to determine with precision, for every temperature and degree of the hygrometer, the quantity of vapour contained in a given volume of air; a result which Saussure could not obtain, notwithstanding his accuracy, because his processes were imperfect.

Method of determining the vapour in air of different temperatures.

This method, which I have already pointed out, consists in taking liquids, from which nothing is separated by heat but water, and boiling them at very different temperatures. For example, sulphuric acid, more or less diluted, and to place the hygrometer under glasses moistened with each of these liquids, and observe the degree at which it becomes stationary. On the one hand, from my experiments, the density of aqueous vapour, which is to that of air as 10 to 16; and, on the other hand, we know the degree of ebullition, or the intensity of each liquid enclosed along with the hygrometer, under the glass

glass jar, and, consequently we have all the necessary data for solving the question. I am at present occupied upon it, and I trust that the results will be interesting for hygrometry.

XII.

Reply to Mr. Farey on the Phenomena of Shooting-stars.

To Mr. Nicholson.

SIR,

THE ardour and *modesty* that characterize Mr. Farey's rejoinder to my remarks on his hypothesis, are but illustrations of the well-known partiality of every projector to his own scheme. In that communication no new facts of importance are stated in support of the hypothesis; nor is the statement I have opposed to it by any means disproved. Under these circumstances, I should not have troubled you or your readers, by any notice of my opinion, had not the paper in question contained some misrepresentations, which I feel it essential to correct. I shall effect this as briefly as possible, since the extension of a discussion of this kind, is only useful when it serves to elicit new facts connected with the object of inquiry; and I shall not notice any farther communication on this subject unless it offer such facts. **Statement; that certain misrepresentions have been made.**

Mr. Farey's first paragraph represents me as having "insinuated" that he had "*exulted*" in the discontinuance of Mr. Foster's obervations. A reference to my paper will prove that this representation is erroneous, neither the word "*exulted*" nor any synonymous expression is to be found in that paper.

The method of observation recommended by Mr. F. viz by two or more observers at several miles distant from each other, &c. is well known, I presume, to every one conversant in natural philosophy; but it appears from the observations of this kind that have been made on the aurora borealis (a phenomenon more favourable to their application, as less transient than meteors **The heights of aurora borealis not well determined.**

meteors) that the heighth and distance of such appearances is not so accurately appreciable, as to warrant a positive conclusion in this respect. The difference in the deductions of very eminent philosophers is, indeed, very remarkable. Mr. Cavendish estimates the height of the aurora at or under 71 miles*. Mr. Dalton and Mr. Crosthwaite at 150 miles†. Euler, Mairan, &c. at various heights, extending in some instances to 300 leagues‡.

The conclusion of Mr. Farey's fourth paragraph, page 299, appears to represent electricity as an hypothetical principle, once employed in explanation of natural phenomena from "*fashion*" only. This requires no comment.

Shooting-stars appear in cloudy as well as clear weather.

I repeat that what Mr. Farey calls "shooting-stars," have been frequently observed in cloudy as well as clear weather, and in the presence of considerable extraneous light, (even during the illumination of the atmosphere by the aurora) and I know not how the observations or assertions of an individual, or *two*, unsupported by concurrent testimony, can be said to "decidedly prove" any thing.

In paragraph 7 I am represented as mistaken in conceiving Mr. F. to have asserted, that the larger class of meteors are not sometimes visible on moon-light nights, &c. Mr. F.'s statement of his hypothesis would have warranted such an assertion, but I did not make it; on reference to my paper (Journal, Vol. XXXIII, page 34.) it will be found I expressly state "SMALLER METEORS" (shooting stars.)

As the smaller meteors move with "*immeasurable velocity*," and Mr. F. has represented them as satellitulæ, I have stated that *if we admit his hypothesis*, we *must* admit that planetary (or satellitic) bodies may move with "*immeasurable velocity*," and become luminous *only* when they dip into our atmosphere. An inspection of the eighth paragraph in Mr. Farey's rejoinder, (Journal, Vol. XXXIV, page 299.) will show his curious distortion of this statement.

If Mr. Howard, or Mr. Forster, concur with, or dissent from my statement, I know not; but in the last number of Mr. Tilloch's Philosophical Magazine, it may be seen that Mr. Forster

* Phil. Trans. for 1790.

† Dalton's Meteorological Essays.

‡ Memoires de Berlin, see also Bertholon des Meteores, tom.ii.p.65.

Forster continues his "meteoric" observations, and they are on that occasion quite *opposed* to Mr. Farey's "*decided proof.*"

If "shooting-stars," or "falling-stars," are terms between which there is a preference, that fact does not appear to be generally known; they are both extensively, and in many cases indiscriminately employed.

One of the large meteors mentioned in my former paper, occurred in a cloudy atmosphere, but it disappeared without passing behind any cloud; from the angle under which I saw it, it appeared lower than those in that part of the atmosphere. Falling-stars I have frequently observed to vanish behind clouds.

Meteors and falling-stars, probably in the lower air.

I have offered no explanation of the *origin* of any meteors, and, therefore, see no relevance in Mr. F.'s concluding observations; he may not have "travelled far enough in theory," but he has entered the region of hypothesis, and whether the term that he has selected "planetary," or "satellitic," it is equally indifferent to the cause of truth. Mr. Farey may dissent from any of the facts I have stated, but to dissent from, is not to disprove.

I remain, Sir,
With great regard,
Your's, &c.
G. J. SINGER.

Princes-street, Cavendish-sq.
April 5th, 1813.

XIII.

The method of steaming or cooking food for cattle. By I. C. Curwen, *Esq**. *of Workington-Hall, Cumberland.*

Extracts from Mr. Curwen's Letters.

IN the prosecution of the system I have practised for some years, of giving cooked food to all animals, the main impediment

* Soc. Arts, 1813.

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pediment has been the cost of labour and fuel; to lessen the one, and simplify the other, have been my constant endeavour; in this at length I conceive I have been completely successful; and that I have thereby removed those obstacles, which were opposed to its becoming generally, and, I should say, universally useful*.

Each of the two steamers which I use has three boxes, containing eleven stone each of chaff, (the husks of corn,) which in boiling gains somewhat more than two-thirds of its original weight. Wheat chaff, which alone I use, is commonly thrown upon the dunghill, as of no value but to augment the quantity of manure. It requires three hours to be sufficiently boiled. The same boiler works two sets of boxes; by the various stop-cocks the steam can be made to work either one, two, or all three boxes of each set.

The usual consumption of fuel is two pounds per stone. I estimate the quantity done every day at the Schoose to be equal to one hundred stone, or thirty-three of chaff, which takes sixty-six pounds of coal. As the expense of coal is not great here, I should not suspect much economy is practised; even at the price of coals in London this would not be above sixteen shillings per week. Two pounds of oil cake are allowed to each stone of chaff. The milch cows and oxen are fed twice, morning and evening, having an allowance of one stone at each time. When taken from the steamer, the food is put into wooden boxes, which are mounted upon wheels to be drawn by a horse. As the chaff and liquor require to stand some time to cool before fit for use, there must be several of these boxes to put the chaff in when taken from the steaming boxes.

* The Society have given a wood-cut. The boiler is globular and set as coppers usually are; it is provided with a feeding pipe, and safety valve, and the steam is conveyed by a main leaden pipe, with branches, into covered boxes, containing the food. Each box will hold 11 stone of chaff. This boiler of 100 gallons is provided with six boxes, three of which, by means of the pipes and cocks, can be worked while the other three are filling or emptying.

The

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The cost of food for each milch cow per day.

	d.
Chaff, two stone, steaming, &c. .	1
Oil cake, four pounds, . .	4
Eight Stone of turnips, 14lb. per stone .	4
Wheat straw, . . .	1
Total	10

The average of milk on a stock of thirty-six milch cows, was nearly thirteen wine quarts for three hundred and twenty days; one hundred and forty-two thousand quarts were sold in fifty-two weeks, ending the 20th of September last, selling price 2*d.* per wine quart. The calves brought from 2*l.* to 5*l.* each for rearing. The produce is nearly half clear profit, estimating the manure as equal to the labour. The milch cows are never suffered to be turned out. To prevent their being lame, some attention is requisite to have their hoofs properly pared, and that they stand with their fore feet on clay.

The condition, health, and milking of the cows fed upon this plan at the Schoose, has created a considerable interest, and called forth particular attention from numerous visitors. The contrast between the condition and milk given by these cows and those fed on grains, as most, if not all, are in and about the metropolis, seems an object well deserving attention.

Most, if not all, the milch cows at the Schoose are in such a condition, that a few weeks feeding after they are dry, makes them fit for the shambles, with very little loss from the first cost.

Compare this with the state of a London dairy ;—what may be the average loss by deaths I know not, but when done milking, their value for fattening is very little; they are so low in condition. As a substitute for chaff and oil cake, I should recommend cut hay. This steamed would make a much superior food; and, I entertain no doubt, would greatly augment the milk, as well as benefit the health and condition of the cows.

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It has struck me, that the sugar wash might be found of great service for boiling the hay.

As I have never seen an instance when cooked food has not produced a striking improvement in the condition of cattle, I am strongly prepossessed in its favour. My representations have had their effect with one gentleman, Mr. Isaac Franklyn, who has a dairy farm at Oxgate, on the Edgware Road, 4½ miles from Paddington turnpike; and who lately had a dairy in Henrietta Street, Cavendish Square. An apparatus made here, is on its way for him.

In order more fully to satisfy the Society, that this statement does not rest on a prejudice, natural to every projector of any plan, I beg to refer to Mr. Tubbs, who on seeing the dairy at the Schoose this summer, was so struck with the condition of the cattle, that he promised to use his influence with Mr. Welling, to attend the meeting in September. Nor shall I rest on the respectable authority of Mr. Tubbs only; but refer also to Sir George Paul, Bart., who will recollect, that before he saw the milch cows, I observed to him, that if the condition of the whole stock did not surpass any he had ever seen, I was ready to admit my system had failed. I need scarcely observe, that the strictest attention is necessary to see the cows always kept clean, and never to suffer the least heat to appear upon their skins, without an immediate application of black soap and water. They are also regularly carded or curried, and care taken in keeping them in a regular degree of temperature. Any considerable change affects their milking. I gave cooked food from October to June, nearly eight months out of the twelve.

No branch of farming is so profitable as the dairy, when properly and well managed. By the mode I propose, I flatter myself there would not only be a great saving in expense of feeding, but also in the depreciation and loss sustained on the capital, with an augmentation in the quantity and quality of the milk; I find that twelve wine quarts of the Schoose milk, will give from sixteen to eighteen ounces of butter, which is little inferior to what can be got at the height of the grass. Much, in my humble opinion, is to be gained both by the individual and public.

Milk in London, from its present price, must be considered as a luxury: Reduce the expense of procuring it, lower the price,

price, and more than double the quantity might be sold, with the greatest benefit and comfort to the bulk of the community. This object is attainable, leaving a handsome and liberal profit to the cow-keeper, and I verily believe would be effected by the system I propose.

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The plan I have now the honour of submitting to the society has been adopted by Mr. Harley, of Glasgow; by Major Ferrand, and the Reverend J. Penny, in the West Riding of Yorkshire, and many others; and in every instance it has been found most completely to answer.

From Mr. C.'s second Letter.

Though great and important improvements have been made in Agriculture in the last few years, it is far distant from perfection even in the most improved districts. The comfort and happiness of the many has always held out to me the strongest incentive for exertion, and no part of my farming operations have yielded me equal gratification as my dairy. The result of what was done at the Schoose last year has confirmed my opinion, that a plentiful supply of good and unadulterated milk is of the highest importance to the comfort and health of the lower orders; 145,000 quarts of new milk were sold in the last twelve months---Milk is now become a necessary of life---Five years ago the amount of the total sale to the town of Workington, containing between eight and nine thousand souls, might be 1,500*l.* per annum, or at two pence per quart, wine measure. 108,000 quarts---From the best information I can obtain, I conceive I am warranted in stating the present sale of milk to be above 5,000*l.* per annum, or 600,000 quarts. Thus the proportion to each individual in the year 1806, would be 20 quarts, in the year 1811, 60 quarts. The truth is, that it was but in partial use. I believe I may assume, that what was the situation of Workington a few years ago, is that of the greater part of the kingdom at present. That species of human food which is produced in the greatest abundance with the least consumption of the fruits of the earth, is a luxury, and not as it ought to be, one of the staple articles of consumption. The time and attention I have bestowed on this subject, have given it an importance in my view, that may surprise those who have only considered it superficially. I am disposed to believe much

 national

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national benefit, as well as national happiness, might be obtained by directing the attention of the farmer to this branch of agriculture, which is but ill-understood. You will pardon me when I say, that even the dairies in and near the metropolis, are under most defective management. According to the usual rate at which improvements travel, and get into general practice, it will require many years to introduce animal cookery. It proposes several objects. Of one I have no doubt, a great increase of milk of a much better quality. I think in many situations it must contribute to reduce the price at which it is at present sold; when a dairy is properly attended to, two-thirds of the cows now kept would give a larger quantity of milk. That instead of there being a most serious loss in the condition of the stock when their milk failed them, they would be very nearly in a state fit for the butcher. The loss of capital to the cow-keepers, is a very serious drawback from the profits of the business; and were this the only object, it would be well deserving of attention.

I should propose that the society should hold out some encouragement to induce the cow-keepers to visit the Schoose farm. If the statement I have made be found erroneous, the expences of the persons who may be induced to undertake so long a journey, shall be at my charge. All my anxiety would be, that they should bestow the time necessary fully to comprehend the plan. If the testimony of thousands who have seen the Schoose warrants confidence, I may assume it: nor is that the only ground; it has been also tried at Glasgow; and in various parts of the West-Riding of Yorkshire, with the most complete success. The price of grain renders the stoppage of the distilleries absolutely necessary; this will deprive the cow-keepers of a considerable portion of food for their cattle. In order to prove to them what might be expected from steamed hay, I am trying four pounds of clover hay, boiled with chaff, instead of two pounds of oil-cake; I am sanguine it will answer. The milch cows drink the liquor in which it is boiled with great avidity. When hay could be afforded in greater quantities, any proportion of liquid might be had; by steaming, two thirds is added to the weight. I have, in various instances, in the course of the last twelve months, sold to the butcher, at or very near prime cost, cows

that

that were giving from three to four quarts of milk at the two meals. The cost of keep being estimated at ten-pence per day, when the quantity does not exceed five quarts there is no profit, and the getting rid of them is an object.

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The amount of milk sold in the metropolis is calculated to be 1,250,000*l.* annually, or 60,000,000 of quarts, this, supposing 1,000,000 of inhabitants, allows sixty quarts to each individual, equal to what is consumed per head in the town of Workington. But it will occur to those who have any acquaintance with the metropolis, that milk is consumed only as a luxury; and is not in use with the lower classes; indeed, it is in inverse ratio; and, instead of being the cheapest, is the dearest food. Supposing the produce of a milch cow, fed with grains, &c. to be ten quarts per day, for 320 days, or 3,200 quarts, it would require 18,750 cows, to give the quantity of milk sold; a pretty strong proof of the adulteration which takes place, as I do not suppose there is near that number kept.

The cost of feeding on steamed hay, I should suppose to be nearly as follows:

	s.	*d.*
One and a half stone of hay, at 6*l.* per ton - -	1	0
One stone dry do. 14lb. to the stone - - -	0	8
Steaming, labour, &c. - - - - -	0	4
	2	0

If I recollect right, 2*s.* 6*d.* per day, was Mr. Welling's estimate of the cost of feeding milch cows some years ago.

From what experience I have had at the Schoose, I estimate the produce of each milch cow at twelve quarts per day; this would add 12,000,000 of quarts, on the number of cows supposed to be kept,—say,

	L.
12,000,000 of quarts at 4*d.* - - - -	200,000
Suppose only a saving of 2*l.* per head, in the number of cows, from superior health and condition - - - - - - -	36,000
	236,00

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I believe I should be warranted in stating the actual loss of the dairymen at 5*l.* per head on his stock, in the neighbourhood of London, by their present mode of management.

Suppose my estimate correct of 18,750 milch cows, valuing them at 28*l*, one with another, would amount to 337,500*l.*

If I have succeeded in my endeavours to impress upon the Society the magnitude and importance of the object, I shall feel I have done some service.

I have the honour to be, with great respect,
Dear Sir,
Your obedient and humble Servant,
I. C. CURWEN.

Workington-Hall, Nov. 15th, 1811.

To C. Taylor, M. D. Sec.

Letter from Mr. Franklyn on the same subject.

I feel it a duty incumbent on me, as it may benefit the public at large, and likewise as a tribute of grateful respect to my honoured friend, Mr. Curwen, who first recommended to me the use of steamed food for my cattle, to inform you, that I have made a long and decisive trial thereof, and have found it to answer, both in respect to the carcase and milk of my cows. They daily increase in quantity, and the quality of the milk is far superior to any I ever saw.

I give steamed food once a day to my horses, namely, in the evening when they have done work, and I find it to answer much better than dry food.

I was accustomed to feed my cows with grains, hay, and green food, but since I have given them the boiled hay, they have not had any grains nor green meat, and I find the cost of their keep to be less than when I fed them with grains. In short, I feel myself so well convinced of the advantages resulting from the use of warm food, that I do not intend ever again to have recourse to grains.

The liquor or decoction from the hay, which is taken from the

the bottom of the steam boxes, is greedily drank by the cows before they begin to eat the hay.

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I am,

Dear Sir,

Your most obedient Servant,

ISAAC FRANKLYN.

Oxgate Farm, Edgware Road,
Welsdon, Middlesex, March 16, 1812.
To C. Taylor, M. D. Sec.

Estimates of the difference in expence of feeding twenty-eight Milch Cows on grains, &c. and on steamed food, each method tried for one week.

Feeding with grains, &c.

	l.	s.	d.
Thirty quarters of grains of eight bushels each, at 4s. - - - - - - - - -	6	0	0
Cartage, &c. - - - - - - -	2	10	0
Seventy trusses of rowen, or second crop of hay, at 2s. 3d. each truss weighing four stones or 56lbs.	7	15	6
L.	16	5	6

The above is, within a fraction, 1s. 8d. per cow per day.

Feeding with steamed hay and rowen.

	l.	s.	d.
Hay steamed, forty-two trusses, at 3s. 1d. - -	6	9	6
Mens' wages, chaff, cutting and steaming - -	0	15	0
Expences of fire - - - -	0	7	0
Seventy trusses of rowen, at 2s. 3d. - - - -	7	15	6
L.	15	7	0

This is, within a fraction, 1s. 6½d. per cow per day.

The balance in favour of the steamed hay is 18s. 6d. per week, besides the better condition and value of the carcases of my cattle, the great increase of milk and superiority of its quality. I deduct three pence per truss from the present average price of the hay used in steaming, on account of the

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the cartage and market expences which are saved by its being consumed at home. I put 1*d*. per day for fire, though I do not believe it costs me any thing, as the water from the steam boiler serves to clean the milk utensils, for which the firing used to cost me as much; besides which I apply the steam to many culinary purposes, having a pipe communicating from my steamer to my kitchen.

ISAAC FRANKLYN.

SCIENTIFIC NEWS.

Geological Society.

April 2*d*, 1813.

W. H. Pepys, Esq. Treasurer, in the chair.

The reading of a memoir by Mr. John Farey, Sen. on the Ashover denudation in the county of Derby, was began.

The first part of this paper consists of minute local observations, incapable of abridgment, relative to the inosculation ridges, the Basset ridges, the partial incurvation of the beds, and the ascertained or supposed faults.

April 23*d*, 1813.

The president in the chair.

Thomas Gregory, Esq. of Bayswater,

Thomas Botfield, Esq. of Hopton Court, near Bewdley, were severally elected members of the Society.

A notice by the Rev. J. J. Conybeare, M G. S. relative to the state of Tintagel, in Cornwall, was read, and thanks were voted for the same.

The slate quarries of Tintagel are situated close to the sea, about six miles N. W. of Camelford. They are worked on a large scale, and are celebrated for the excellent quality of the roofing slate which they afford. No dykes of granite or of porphyry have been observed in this rock, but there are veins which afford quartz, rock crystal of great transparency and

and beauty, calcareous spar, chlorite, and, in some instances, adularia. The slate of Tintagel appears to bear a near resemblance to that of Snowden, and, like it, occasionally presents the impressions of bivalve shells.

The reading of Mr. Farey's paper on the Ashover denudation, was concluded, and thanks were voted for the same.

This portion of Mr. Farey's paper contains a detailed account of the several strata represented in the map and sections, beginning from the lowest of those that are known.

The fundamental rock of Derbyshire is the fourth limestone. It is supposed to lie at the depth of about 350 yards below the level of the river Amber, in Ashover valley. It rises towards the surface under Matlock valley, and actually bassets in Griffdale. The thickness of this bed is unknown; but as the deep vale of the Dove is entirely excavated in it without discovering the bottom of the bed, its thickness cannot be less than 350 or 400 yards. It is generally a pure calcareous free stone, of a whitish yellow colour, disposed regularly in very numerous strata. These consist either of very white marble, or of aggregations of small rhombic crystals. Towards the top it is very compact and porcellaneous. Few of the beds are without organic remains—in some are found small anomia, in others entrochi, or turbinated shells, cornua ammonis, nautili, and branching coralloids. This rock is superficially cracked, so as to present a columnar appearance—beneath it is much rent, and abounds in shakehole and large caverns with water swallows. Some of the fissures connected with the surface are filled with clay, sand, and quartz pebbles. The veins are filled principally with calcareous spar, heavy spar, and fluor spar. They also contain in their upper part galena, calamine, manganese, red iron ore, white China clay, and stratin.

Upon the fourth limestone lies the third toadstone. The most eastern basset of this rock is at Bonsall upper Town. Its thickness in different parts is very various, from four feet to 80 yards. Its usual appearance is that of a cavernous strong mass, of a dirty purplish brown hue—often it is of a dark blue colour with shining specks as hard and sonorous as cast iron—also of a light green or bluish grey colour, and rarely it appears of a gritty yellowish stone called Davestone. Its structure, when recent, is amygdaloidal, the cavities being filled with green or white

white globules of calcareous spar. The veins in the limestone, above and below this stratum, have rarely, if ever, broken through it, but rents proceed from these into the top and bottom of the toadstone, in which galena and the usual veinstones are sometimes found.

Third Limestone. The most eastern basset of this rock, in the line of section, is on the western slope of Massen Low. Its average thickness near Matlock is about 80 yards. Its colour varies from grey to brownish black. It includes several beds of swinestone, with layers of dark grey inodular chert. Its organic remains are numerous, and it abounds in mineral veins that afford galena, calamine, and blende included in calcareous spar and heavy spar.

Second Toadstone. The most eastern basset of this rock is in Matlock high Tor. Its average thickness is about 80 yards. Its colour is yellowish or bluish grey. Some of its beds are magnesian limestone. Its principal organic remains are entrochi. It contains metallic veins of galeva, calameric, and, it is said, of copper.

First Toadstone. The first regular basset of this rock appears to be in Matlock high Tor. Its average thickness is about 28 yards. Its general characters differ little from those of the third toadstone, except that it seems disposed in more regular beds or strata.

First Limestone. The average thickness of this rock is 60 yards. Its usual colour is lightish grey. Near the top it encloses beds of swinestone interlaid with dark or striped chert. The organic remains of this rock are anomia, entrochi, nautili, and other shells, together with many coralloids. It abounds in caverns and water swallows, and in numerous metallic rake veins, or long vertical rents. Massive fluor (Blue John) and elastic bitumen occur in this rock.

The greator Limestone Shale. The average thickness of this rock is about 150 yards. Its general character is that of a black or dark brown shale, inclosing beds of a soft yellowish sandstone, and of a dark blue limestone: also thin beds of clay, iron, stone, and septaria. Its organic remains are not numerous, consisting chiefly of anomia, mya, helices, and a few vegetable impressions.

First

First or Millstone grit. The average thickness of this rock is about 140 yards. It is generally a white or yellowish coarse grained freestone in thick beds; but at the upper part of the rock is a considerable thickness of soft micaceous thin beds. Its organic remains are large reeds and flags, and occasionally corralloids of a hone-like appearance.

Coal Formation. This lies on the Milstone grit, and consists of eighteen beds of grit and of shale, the aggregate thickness of which is 706 yards, and presents the usual characters of the independent coal formation.

Abridged Account, or Abstract of the "Elements of Crystallography, by F. ACCUM," *in* 1 *vol.* 8*vo. Pp.* 391, *with* 4 *copper plates and numerous wood cuts. Longman and Co.*

THIS author in his preface remarks, that considering how much the philosophy of the mineral kingdom has of late been advanced and perfected, by the application of Haüy's theory, and the light it has thrown on some of the most obscure branches of Mineralogy, it is surprising that no English work on the structure and formation of crystals has yet appeared; whilst on other departments of the science of minerals, many excellent works have been published. He offers the present Treatise for the purpose of initiating into the principles of crystallography, those who possess no previous knowledge of it.

As the theory which explains the production of crystalline forms, and their metamorphoses, abounds in mathematical and algebraic calculations, and cannot be studied with ease and success, by such as are unacquainted with the mathematics; he has thought it desirable, as has before been announced in this Journal, to prepare sets of models to which reference is made in this work.

The dissected models, are so constructed, that they can readily be taken to pieces, and put together again in various ways, to give a distinct conception of the laws of that geometry of nature which are followed by the integrant particles of crystallis-

able

able bodies when they combine, and the orderly arrangements of which produces symmetrical crystals. These will, no doubt, be very useful, particularly to such as are unacquainted with mathematics, and might, on that account, be scarcely, without such assistance, enabled to study this interesting department of science. The designs which accompany the work, give the complete figures, and render the models not indispensable, but supplementary, for more readily understanding the subject.

The wood-cuts, which amount to upwards of 100, and plates introduced into the work, in illustration of the laws of the science, render the crystallographic models superflous to those who are familiar with geometry and linear perspective. The graphic designs of the crystals have been traced by the method of projections; the entire lines represent the edges or outlines of that part of the solid immediately turned towards the observer; and the dotted lines express those edges which are in the opposite part of the solid, and which of course the observer could not see, unless the solid were diaphanous.

The work is divided into 4 parts, and each part is again subdivided into sections. The first part of the Treatise is introductory to the 3 following, and is divided into 4 sections. It treats on the general nature of the process of crystallisation—the disposition of crystallisable materials of different kinds, to assume a certain number of geometrical forms, peculiar to them, and no others—the rectilineal interior structure of crystalline solids, their increase or growth, stating it to take place in such a way that eve y thing which existed at each period of growth of a crystal, remains fixed, and so as to present on all sides a basis as it were for the crystallisable materials which arrive for containing the edifice, and thus contrasting it with the growth of organic beings, and general definition and objects of the science of crystallography.

The 2d section exhibits the practical means by which crystallisable materials are made to assume symmetrical forms, the crystallisation of saline, metallic, and other bodies, as it is induced by the processes of solution, fusion, sublimation, chemical affinity, &c. The circumstance which influences the crystalline processes—the ratios of crystalline energies in different bodies—its admeasurement and other practical statements relating to the artificial crystallisation of bodies.

The

The 3d and 4th sections give a rapid view of the geometrical definitions employed in crystallography—the admeasurement of the angles of crystals, with a description and use of the goneometers of Wollaston and Carangeau employed for that purpose.

The 2d part commences with a general view of the constitution of bodies, and a sketch of the theory of crystallography of Haüy—it is there shown, that all crystals, however complicated their forms may be, contain within them a geometrical nucleus, called by Haüy the *primitive form*, which has, under certain restriction, an invariable shape in each chemical species of crystalline solid, and which may be extracted out of all of them, by a skilful mechanical dissection.

The 2d section of this part, shows the mechanical dissections of crystals, to develope the primitive forms, their nature, their number, and situations in different bodies, and general inferences relating to the mechanical analysis of crystalline bodies in illustration of the theory. The dissections are illustrated by graphic projections, and rendered farther striking to the senses by dissected models, of which references are made.

But the primitive forms of crystals which are considered as bodies of a constant geometrical form, inscribed symmetrically in all the crystals of one and the same chemical composition, are not the ultimate result to which the mechanical dissections of crystals may be carried.

The 3d section shews, that the primitive forms of crystals are divisible in a direction parallel to their faces, and sometimes also in other directions, and this in the latter case must produce solids, which differ in shape from the primitive crystal to which they belong, and these solids, thus obtained by different modes of dissections, are called, by Haüy, the integrant particles of crystals. To illustrate this statement, numerous examples are given. The sketches and dissected models advanced on this occasion are the following: the rhomboid of the tourmalin—the hexahedral prism of phosphate of lime---the cube of fluate of lime, &c. &c.

The 4th section, in a similar manner, elucidates the laws of decrements of the structure of crystals, or the laws of crystalline architecture, according to which are produced those arrangements of particles or regular coverings which disguise, under such various forms, one and the same primitive form or nucleus, and which give rise to such an infinite variety of crystalline solids as are met with in nature. The laws of decrements are detailed in the synthetic form, and illustrated by leading examples, all of which are rendered legitimate by the methods of analysis, and synthesis by appropriate models, both solid and dissected, so as to interest their mind, and to leave positive and exact ideas in the memory. The models referring to this part of the work are the following: the rhomboidal dodecahedron, considered as a secondary form originating from a cube as the primitive solid; the production of an octahedral solid considered as a secondary crystal, originating from a cube as a nucleus—production of the irregular pentagonal dodecahedron originating also from a cube of the bi-pyramedal dodecahedron, originating from a cube as a primitive solid.

In part 4th the author has considered the difference which exists between structure and increment, as relating to the production of crystals, and the alterations to which the symmetry of crystals is subject: and these are succeeded by the electricity of crystals—their double refraction,—principles of nomenclature—amorphous crystallization,—table of crystalline forms, &c. &c.

CAPTAIN Lisiansky, one of the celebrated Russian circumnavigators, who a few years ago commanded one of the Russian ships in company with Captain Krusenstern round the world, has lately published, at St. Petersburgh, his curious and interesting voyage, in the Russian language, which I understand the author himself intends to publish likewise in English. The work is already translated, and the materials necessary for publication are in great forwardness. I am informed that it will be more complete in English than it is in the original, as it will contain a greater number of drawings, plates,

charts,

charts, tables of longitude and latitude, variations of the compass, those of the thermometer, barometer, &c. which are intended to be hereafter added to the Russian work in a supplementary volume.

PRECURSOR to an Exposé on Forest Trees and Timber, &c. (intended as a preliminary introduction to a more enlarged work upon the same subject) as connected with the maritime strength and prosperity of the United Kingdom and the provinces: with an Appendix, containing an outline of the dimensions, force, and condition of the British Navy, compared with that of the enemy. Humbly addressed to his Royal Highness William Duke of Clarence, Admiral of the Fleet, &c. &c. &c. By Captain Layman, of the Navy. 8vo. 70 pages closely printed in bourgeois, with 1 copper plate, and many tables. Asperne.

This work abounds with very rational observations, upon facts which are of the highest interest to the Empire and every individual in it.

TO CORRESPONDENTS, &c.

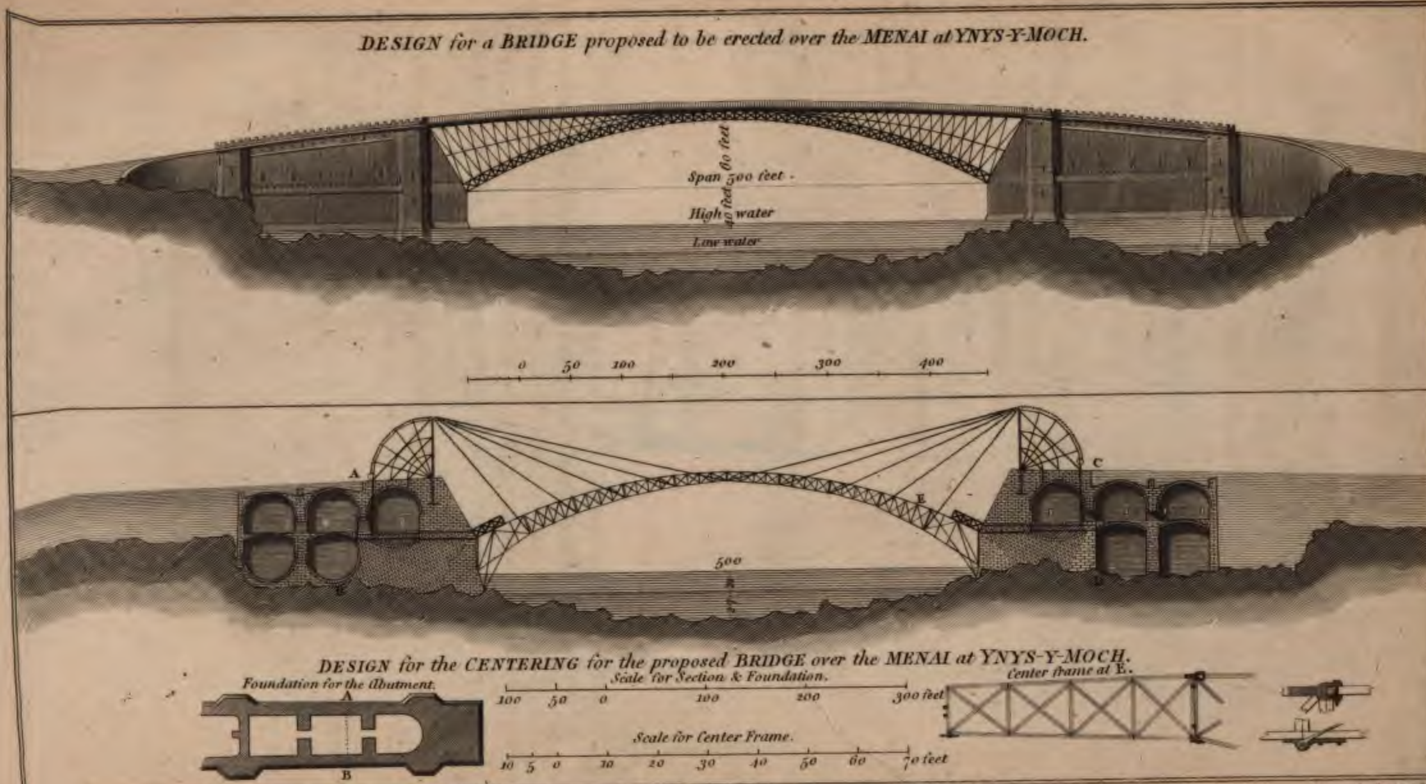
Circumstances have prevented the insertion of Dr. Armstrong's communication later than was intended.

I shall pay attention to the remark with which M. D. has favoured me in his postcript. It does not appear to me, that any practical *advantage would result from plano-convex or plano-concave spectacles, in preference to other lenses of like foci; neither do I apprehend that such lenses ought to be charged at an higher price, supposing the workmanship and materials to be the same.*

L. O. C.'s Reply to Mr. Kerby came too late for the present number, but will appear in the Supplement.

The Engraver, by an accident, has spoiled the plate of Mr. Telfo·d's bridge, which could not be re-engraved in time. It will appear in the next number.

The Supplement to Vol. XXXIV will be published in the course of the month.



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A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JUNE, 1813.

ARTICLE I.

Discussion on the Cause of the Figure of a Man, which was formed on the Ice of the Pond in Halnaker Park. In a Letter from Mr. ROBERT HARRUP.

To Mr. Nicholson.

SIR,

I TAKE the liberty of sending you a few observations on the remarkable appearance in the ice on the pond at Halnaker House, so minutely described in the last number of your Journal;* at the same time offering some remarks on your ingenious explanation of that singular phenomenon. If I differ in opinion from a philosopher of your abilities and experience, it is with the greatest diffidence. If my conception of the cause is erroneous, I hope you will have the goodness to set me right.

Observations on the remarkable appearance on the ice in the pond at Halnaker House.

You are disposed to think, that the effect was caused by the developement of heat in the body, occasioned by the putrefaction or chemical change which must have taken place. I cannot perceive how this supposition is to be maintained, when it is

The body of the drowned man did not probably give out heat;

* Philosophical Journal XXXIV. 301.

is expressly stated *that the body was quite stiff, and showed no signs of putrefaction.*

or if it did it is not likely that it could have been sufficient to operate through five feet of water.

This fact might, indeed, have been inferred from the low temperature in the month of December, and the other circumstances unfavourable to that process. Aware of this, it is observed in the explanation that putrefaction may have proceeded very slowly, on account of the coldness of the water and the want of communication with the external air. But admitting the supposition in its fullest extent, that a slow putrefactive process was going on, sufficient to develope as much heat as would occasion a small degree of expansion in the water in contact with the body; would not that additional heat be entirely lost, or more properly, given off, in all directions long before it arrived at the surface, through a depth of five feet of water at that low temperature? Even the greatest possible degree of heat given out by a human body in the highest state of putrefaction, appears to me insufficient to produce all the phenomena, or to affect so large a quantity of water in any perceptible degree.

The smoothness of the ice over the figure is ascribed to *repose*, and the softness of the other ice to *agitation* during freezing.

Why was the ice of the figure smooth, while the other parts were crumbly and soft? It is evident, whatever the cause may have been, that that part of the water directly over the body, must have been in a state of rest on the surface when the frost commenced, while the other parts were subjected to a gentle agitation. This, certainly, could not have been caused by any degree of heat.

Opinion, that oily matter rose from the body, and defended the surface of the water above it from being agitated by the wind:

I am inclined to think, that some time after the body sunk to the bottom of the pond, the water, by insinuating itself through every pore, would gradually dislodge the oleagenous and greasy particles adhering to the skin, and also whatever there might be of that nature under it—these would, undoubtedly, find their way, by degrees, through the clothing and rise to the surface. Now it has been well known, from the time of Dr. Franklin, that oil or grease, in a fluid state, when floating on the surface of water, has the property of rendering it smooth and incapable of being affected by the wind. This I apprehend was the case in the present instance, and which affords a ready explanation of all the phenomena. The oily matter as disengaged from the body would rise in a perpendicular direction and form a thin stratum on the surface, which, although diffused, would still continue

continue in greater quantity directly over its source, than on any other part. From the roughness of the ice it is evident, that when the frost came on, the surface had been gently agitated or rippled by the wind every where, excepting that place occupied by the oily film, which would necessarily be smooth and at rest. It is also evident, that this part would not congeal quite so soon as that in a state of motion, as it has been long known, that water when at rest may be cooled many degrees below the freezing point without losing its fluidity, but on the slightest agitation the congelation is instantaneous.

it is inferred from the roughness of the ice that the wind had agitated the water previous to the congelation: except where defended by the oily covering.

Why the figure of the hat was not represented in the ice, is a further corroboration of the foregoing opinion, as any oily or greasy fluid could not readily find a passage through a substance of so close a texture as felt.

The figure of the hat wanting;

The white line bordering the figure seems to have been occasioned by the abrupt termination of the confused crystallization, and the transparency of the ice by the tranquil state of the water in the act of congelation, by which the extraneous particles, with which it was mechanically mixed, would be deposited.

the white border; and the transparency accounted for.

With respect to the snow first covering the figure, and afterwards disappearing it might either have been swept off by the winds, or, more probably, melted during the day when exposed on so smooth a body, while that on the other parts would be protected by the inequality of the surface.

Why the figure was bare of snow.

I am,
Sir,
Your obedient humble Servant,
ROBERT HARRUP.

Cobham, April 7th, 1813.

ANNOTATION. W. N.

Upon a subject which still seems to be obscured by difficulties, I should, probably, have been inclined to leave the farther discussion to my correspondents, instead of making any remarks myself upon the ingenious speculations of Mr. H. if I were not led by his invitation to point out a few circumstances which appear to militate against his inductions.

Some facts which seem to indicate that the phenomenon was not occasioned by oily matter.

 1. When

1. When oil is poured or dropped on water, it spreads on all sides, with astonishing rapidity (I suppose 5 or 6 yards in a second) and smooths the surface by suspending the effect of the wind in forming the primary waves; but it does not destroy the larger waves or the swell, nor prevent the undulations from being transmitted from the rougher parts into the smoother.

2. The smoothed surface is carried gradually to leeward, and after a short time, perhaps two or three minutes, a large pond, upon which oil has been poured, will become as rough as at first.

3. The man was drowned about a fortnight before the frost came on; which seems a long time for the *oil* to have been rising and confining itself to a definite space in so small a pond: and if we suppose some process of change to have developed the oil just at the time of the frost, and not before,—this change would also be likely to be attended with change of temperature. And even then, since oil spreads so quickly, and has so transient an effect, and Mr. H.'s hypothesis supposes a wind to have prevailed, it appears scarcely to be admitted that it could have occasioned that permanent and precisely bounded quiescence which his reasoning demands.

II.

On the Effects of Twenty Thousand Zinc and Silver Plates, arranged as an Electric Column. By Mr. GEORGE JOHN SINGER, *Lecturer on Experimental Philosophy.*

An electric column of discs of silver zinc and papers

THE remarkable properties of the Electric Column, invented by Mr. De Luc,* rendered the construction of that instrument on an extensive scale a desirable object. Trials were previously made on the effect of various methods of combination, to ascertain the most efficient arrangement. That which has been here employed consisted of *two* discs of paper interposed between each pair of metals; one disc being pasted to the silver, the other disc unconnected with either metal.

consisting of one thousand

A series of one thousand pairs of plates thus constructed, each

* Philosophical Journal, XXVII. 81.

each plate having a diameter of 5-8ths of an inch, occasions the gold leaves in an electrometer, (whose cylinder is one inch and a half in diameter), to strike the sides, at intervals of from half a second, to two or three seconds, varying at different periods. pairs of 5-8ths of an inch diameter, constantly affected the electrometer,

The extremities of the above column being connected with the opposite coatings of a Leyden jar, containing about 50 square inches of coated surface, a charge was communicated to it capable of affording a very evident spark. The period of contact required for the production of this effect, was seldom less than one minute, and no advantage appeared to be obtained by its continuance beyond five minutes. and charged a jar, which gave a spark,

The charge thus communicated, even at its maximum, produced but a very slight shock, the sensation being only perceptible at the points of contact. and slight shock.

Twenty columns similar to the preceding were now arranged, so as to constitute a series of twenty thousand groups of silver and zinc, separated by double discs of paper. In this apparatus, beside the two extremities, and a central point, there are 18 intervening situations, at which an electrometer may be applied, the whole apparatus being insulated. Twenty columns, or 20,000 pairs,

The power of the apparatus was such as to affect pith ball electrometers; several of these were employed with balls one-fifth of an inch diameter, suspended by fine threads four inches long. The electrometers at the extremities of the apparatus had their balls separated by its action, frequently to the distance of two inches and upwards, and their divergence was rarely less than one inch and a half; the electrometer at the centre was not affected. Electrometers applied at intermediate points, between either extremity and the water, exhibited various degrees of divergence, diminishing in intensity in proportion to their proximity to the central point. had a considerable effect upon the pith ball electrometer, &c.

When either extremity of the series was connected with the ground, that extremity became the neutral point, and the annexed electrometers divaricated, with progressively increasing intensity, towards the opposite end, where the original divergence was considerably increased; but the electro-motion in an apparatus of this extent is so slow, that some minutes are required to produce the maximum of effect. The electro-motion slow.

A communication between the opposite extremities of the series Sparks seen at the opposite extremities of the column.

series was produced by a fine iron wire, separated from actual contact by a slight layer of varnish; a succession of small bright sparks were thus produced, they were most apparent when the point of the wire was drawn lightly over the varnished surface.

A thin jar of 20 square inches, gave a considerable shock.

A very thin coated jar being interposed between the opposite ends of the column, it instantly received a charge, which, by continuing the contact, progressively increased, and in ten minutes became so powerful as to convey a disagreeable shock, felt sensibly in the shoulders, and by some individuals across the breast.

Paper was perforated.

Battery.

Jars of progressively increased thickness and of various sizes, were charged in the same way, and apparently to similar intensity; but none of the charges thus produced had sufficient power to perforate a card; thick drawing paper was the greatest resistance the most powerful of them could overcome. Several jars, connected as a battery, were charged in the same way as a single jar, but much longer time was necessary; the effect of the shock, and its power of perforation, were not greater than with a single jar.

The single jar fused one inch of very thin platina wire.

The thin jar first mentioned, (which contains about 50 square inches of coated surface,) when charged by 10 minutes contact with the column, just fused one inch of platina wire, $\frac{1}{5000}$ of an inch diameter. The same effect could not be produced by thicker jars of the same size.

The electrical power has not diminished in five weeks, though it has varied.

The electrical power of the column has not diminished by five weeks constant activity, though, during that period, its intensity has frequently varied; in some instances its power has been sufficient to produce (though faintly) the configurations of Lichtenberg.

No chemical effect has been produced by this column.

Various saline compounds, tinged with the most delicate vegetable colours, have been made the medium of communication between its extremities, and the contact preserved for many days; similar experiments have been made with metallic solutions, but in none of these trials has the slightest trace of any chemical effect appeared.

Columns of this description have continued in undi-

The cause of electric excitement in the column appears to be permanent. I have some that have now been constructed upwards of two years, and their power is in no way diminished; in

cases

cases where the contrary has happened. I conceive the presence of too much moisture, and the consequent oxydation of the zinc surfaces must have been the deteriorating cause.

minished power for two years.

Prince's Street, May 10, 1813.

III.

On the Wood and Bark of Trees much magnified. By Mrs. AGNES IBBETSON.

To Mr. Nicholson.

SIR,

IT was my intention that this letter should have concluded all I had to recount on the roots of fruit trees, and also of the same part in those plants which rise yearly from the earth; but I find it impossible, (let my study of the subject have been ever so perfect,) to write properly on it, without having the object before me. This, though it makes my labour excessive, also, I hope, makes it more exact. The necessity of reviewing every part as often as I write on it, has made me correct many a fault, and destroy many an error, that would otherwise have appeared in my phytological review; but it will also impede my giving the subject I intended, in *lieu of which* I shall produce an exact delineation of wood, extremely magnified, both in the root and in the stem; pointing out the variation to be found in *the two*; the form of the sap vessels; the construction of the bastard vessels; the management of the silver grain; and shew also how the net is contrived in which the sap vessels are inserted, and many circumstances that will draw their sources from this subject; and conclude my letter with dissections of the formation of the bark; shewing how impossible it is that that part can possess any returning sap vessels; since, *except* those of the inner bark, (which their thick liquid so exactly identifies,) all the vessels run in a contrary direction.

To delineate the wood much magnified.

I

Formation of the wood.

I shall now begin with the dissection of the wood of the root. It may be said to consist of four parts; fig. 1. The sap vessels, which are large simple cylinders, without any division; B. the net in which they are inserted; C. the bastard vessels; D. the silver grain which marks each yearly circle; the sap cylinders engross much the largest part of the root; for though it is not such an *absolute sponge* as the radicles; yet the apertures are very closely placed, so as often to break into each other; see fig. 2, when the layers are cut very thin, to form transparent images of the parts. It was with great astonishment that I discovered the excessive motion of the root, and the folds which appertained to it; but had I then been apprized of the nature of the net, I should have known that it was but the *necessary* consequence of its *formation*; upon placing some very old vegetable cuttings of the root in my sliders, and exposing them to the highest magnifying powers I possessed, I found that almost every part of the net was composed of spiral wire in its cases; and as the cases in many parts had broken away, all the interior was become visible; this has made me dissect many woods, and I find that it is the *same in all*; that not only the greatest part of the net is so formed, but that the spiral vessels surround each *sap vessel*, first extremely tight at the edge, reducing or enlarging the aperture, and then a little beyond it, forming a kind of scollop, besides large coils of the same, passing from one aperture to another, see A, EE, like those which surround the bastard vessels, and mark all the folds. The wood may be truly said, therefore, to be little else but spiral wire, and to be perpetual motion in the interior, the effect of the quantity of those vessels that pervade it. This shews of what consequence dissection is, and that, by banishing all *imaginary causes*, and letting *nature herself* shew her own works, and be merely the transcriber of the information she gives, by degrees every cause will be discovered; when I found out that the *root* possessed such motion, I had no idea the net was formed by the spiral *wire*; but I was resolved to take each *thread to pieces*; to see and comprehend its *texture*; and, surely, this perpetual motion, aided by *capillary attraction*, is quite sufficient to account for the rising of the sap.

Net of the wood all spiral wire.

Root; perpetual motion in the interior.

Capiliary attraction helps

Three years ago, I dedicated a whole *winter* to the study of capillary attraction; and I found that the smaller the vessels, the

the stronger and higher was the impulse above the level; when I had procured glass pipes, from 150 to 200 of an inch in bore, the water rose to 19 inches, and was only then stopped by the roughness of the interior, as I discovered upon breaking the pipe. Now all pipes made *by man* will have *this defect*; I am convinced, therefore, that we know not half the force of capillary attraction, as when the pipes become so diminutive, the roughness is of such immense consequence as to *stop the flow of liquid*; but, in pipes formed by *nature*, there is no *such defect*, and the liquid will therefore rise *much higher*. I am the more convinced of this, as the *film of the water* rose near an inch beyond the before-mentioned height, caught on the points of the excrescences and roughness of the interior of the vessel, which, when exposed to the large powers of the opake solar microscope, looked most prominent. But when to capillary attraction is added the perpetual motion of the root, the strong impetus this must give to the liquid, the additional force the sap vessels gain by the variation of pressure of the spiral wire round them, changing with each alteration of the atmosphere; and that, as the impetus must be renewed at each *prominent shoot* in a tree, it requires the capillary attraction only, to convey it from shoot to shoot: weighing all these causes well, there can, I think, be no doubt that they are all sufficient for throwing up the sap in trees; but this is merely a hint the motion of the wood has drawn from me, and to be aided, I hope, by future discoveries, for which no diligence shall be wanting, which will, I doubt not, soon explain the enigma, to the satisfaction of all.

to throw up the sap.

Interior roughness of the pipes.

I shall now turn to the formation of the silver grain, in which there is a part that has greatly puzzled me. I always perceive that whenever I cut a transparent piece of the wood horizontally, a very diminutive row of increase appears at each silver grain; this must certainly be an *addition* as it is *fresh made* wood; and this alone will account for the stripes that always appear between each yearly circle. They are from 10 to 12 in number, they certainly are not in the alburnum, for that shews no marks but what would naturally be found in every soft matter pressed through a pipe rather twisted; it must, therefore, be made afterwards; but it is impossible the wood should go on increasing constantly at this rate, or the width would be infinitely greater between the yearly rows; but if the new wood (as soon as com-

Additional piece to the silver grain.

plete)

plete) is measured, it is much narrower than when the size is taken a few years after; some addition, therefore, must have been made. This is of consequence to discover, as it all helps the growth and motion, and teaches us how much more a tree is a moving machine, than we have ever considered it to be. The coils that appear round the sap vessels will not shew themselves in a vegetable cutting taken with the instrument, as that tears them all off; but when cut with a sharp knife or razor, and left a little time, they will rise up, and the smallest magnifier will shew the roughness they create, and the large coils that pass from one sap vessel to another, especially in the *chesnut* and *acacia*. In the latter they can be seen with the naked eye, and they are so *tight* in the layers of wood, that the cutter, can not divest them of them—many of the sap vessels appear to have spirals across them, in 2 or 3 places; but I rather think it is *many apertures* broken into one, which before holding tight at the interior of the circle, are, when released, drawn across the holes from the contracting power of the spiral wire.

Endeavour to discover it.

Acacia and chesnut good examples.

There is no very great difference in the figure of the wood of the root and stem; except that the *latter* possess not so much spiral *wire*; that the coils from one sap vessel to another, are not so large and strong, that the folds in the stem are *hardly observable*, indeed, except in *firs*, I never found any there, and even in them infinitely less than in the root; nor are they, I believe, marked by any spirals, as I see no cross ones sticking from one side to another, which immediately obtrudes on the sight when dissecting the wood of roots; but the stem is perpetually marked by the shooting of the buds, which in trees and shrubs (especially at certain times of the year) is almost constant. These points, therefore, make the principal difference, and balance the more regular motion the folds of the root produce. I shall leave the description of the rising of the bud in the middle of the wood in those plants that shoot each year from the earth, till my next letter, when I give the root of those plants. It is, indeed, one of the most beautiful effects arising from the spiral formed wood, and its consequent motion; it admirably accounts for the different manner the buds shoot, in running *up the wood* instead of across it, as in *trees*; since in the one the spirals are in their cases and confined, but

Difference of the wood in the root, and stem.

Substance of my next letter.

but in these they assist in *forcing up* the plant, keeping the vessels extended that the flower may not be injured, as I shall shew in my next. It is inconceivable by how many marks these two grand divisions in nature are indelibly fixed. I mean between trees and herbaceous plants, &c. their whole formation appears different, not only do the latter form the bud in another part, but their whole existence (the root excepted) is but the growth of a year; but this laboratory is also in a contrary direction. In trees and shrubs, it must be at the bottom of the flower stem, for there all the pieces must be prepared and concocted, and its vessels arranged for the different parts they are to compose; but in plants that form their bud in the root, the laboratory can be no where but in the root alone. I promise myself the greatest pleasure in a most exact investigation of that part. I have already near a hundred different roots with their flowers as they appear in the solar microscope; but I wish also to give a specimen of them as they rise in the wood vessel, and shew how the spiral wire winds round it to keep it open

Two grand divisions in the vegetable world.

But dissection has done more for us than shewing the origin of the flower bud; it has also taught us from whence the leaf buds proceed, and proved how extraordinary the difference exemplified between the two, which will lead me to my last observations on the bark. The leaf bud (as I have before shewn) is formed of the juices of the bark and the elongation of its vessels, it never touches the wood, except receiving the few nourishing vessels that run into it; nor proceeds further in the interior, than the inner bark where it is engendered, and remains till bursting out into leaf; and all the purposes nature seems desirous to effect, by its various precautions, is keeping the juices separate, the sap from entering the precincts of the bark, and the bark liquid from reaching the wood: and the more to effect the separation, all the trees that have a superabundance of bark juice, are divided by a curtain which preserves them from this mutual contamination. Now, we are well acquainted with the softening power of the bark juices—does it not strike the mind, that this piece must be placed in such a situation to prevent the alburnum from being injured, and the wood from being softened and destroyed by its reducing powers? But if this idea strikes the imagination, how much more must it convince the judgment, on examining the matter so applied, and finding, that

Where first growing.

Matter softened by the bark juices.

that though it is really nothing but a piece of wood so placed, (that must originally have been of the same hardness as the rest) but it is converted, (by means of this softening liquid,) to the appearance of a most beautiful white leather. *Then, indeed,* conviction usurps the place of fancy, and we become assured that we guessed right. How strange, then, to alter all this beautiful *arrangement—justified (indeed taught)* by dissection, in order to find a place of circulation for *sap-vessels,* that cannot possibly *require any*; for why *must* they have returning vessels? Is there not a great difference between an animal which, after the first few years, has no increase, and a being that increases from every joint; and may be supposed, therefore, to draw up only those juices necessary for that increase, especially as the sap is the liquid of the earth, not the blood of the TREE, as is easily proved by adding nurture to the ground when the sap *fails,* which soon restores it; besides, how is the circulation to be effected in the eternally decreasing branches of a tree, whose every additional twig must make a variation in the quantity of juices wanted; whereas, it is naturally decreased as it mounts by the throwing out new *shoots* and *branches,* which expend the liquid as it rises. But there are two, I may say, *unanswerable arguments,* that must put an end to the existence of such a system. 1st. If there were such a law as gives circulation to the sap, instead of the power which merely carries up the quantity of liquid wanted for the use of the tree, it would be *universal in all plants*; it would be common to all without restriction. Now, there are a vast number of plants that lose their bark every winter, and renew it in the spring; but when the bark is gone, the inner bark-vessels (full of their juice) are still seen pressing to the wood—but what would become of the returning sap-vessels? They could not do the same, or their quantity would have made a large portion of itself, and filled out the bark, which they certainly do not do; for there are no vessels to be found but those I have mentioned—even the rind disappears in many plants. The vine is one of these, and many other twining plants. All the pentandrian plants lose their bark long before they have formed their seed. Numbers of the syngenesian plants begin very early to lose it; but as it disappears, leaving the rind, few who do not examine and cut plants

No returning vessels in the bark.

Two unanswerable arguments.

No vessels to be found.

plants continually, as I do, would know these little facts that never present themselves to the eye, but require to be sought with a diligence few will bestow, though rewarded with a delight none but those who have felt it can conceive. Numbers of shrubs and semishrubs also lose their bark, and retain the inner bark-vessels only. If, then, we suppose the returning sap-vessels to die away in the winter, what is to become of the sap in the intermediate time? for it is a certain thing that the sap flows all the year; and though, at the time it rises to form the alburnum, it is in greater quantity, and more visible to us, because it then shews itself between the bark and wood; when the bark retires back, the sap fills all the intervening place, and is only prevented from running into the bark by the curtain or division I have before mentioned. It is at this time that trees will bleed their sap, and the quantity that they will yield in that case, plainly shews that it is not their own juices, especially as custom will increase them to an amazing quantity. But the second reason is still more forcible. The sap-vessels, as they mount, are most apparent in every part of the wood; their large open mouths prove at once for what they are designed, and the quantity of sap always found in these vessels, shew most plainly their office. If, then, there were returning vessels, would they not also be visible? But, in the bark of more than twenty trees which I have taken to pieces, fibre from fibre, tracing their direction, examining them in the microscope one by one, so that no vessel could escape me, not a sap-vessel can I find. The inner bark-vessels, identified by the dark liquid that fills them, and their peculiar shape and form, are not to be mistaken for any others; and I believe I may say, that I am now so well acquainted with all the different *vessels* of a *tree*, that I can no longer fail from *ignorance*. But here, except the inner bark-vessels, all proceed in a different *direction* either *round* the tree, or from the centre to the circumference, how is it possible that such large and powerful parts should be invisible? The use of dissection is to correct the work of imagination, or those experiments which have that defect, forcing the juices into channels foreign to that which nature has appointed for them. I have before said, that I *ever found nature* disposed to seek resources in case of any unnatural impediment. I have myself (in several instances

I could

The bark going off in winter.

The large open mouths of the sap-vessels.

Would not the returning vessels be visible?

The use of dissection.

I could mention) proved it. If Mr. K., therefore, stops up the usual current, it may force itself into the bark to find a passage. But this does not teach us what are the directions of its sap-vessels! This is the reason of my disliking all these experiments (at one time so much the fashion) and which, I am firmly persuaded, is the grand cause of the mistakes made in phytology, and the reason why so little is known. A wrong road was marked out, or it was taken too early, before a foundation was laid. I am only a dissector; I pretend to nothing but delivering faithfully what I see, without exaggeration. If this matter was to be decided by argument or general knowledge, I should never attempt to contradict a gentleman of Mr. Knight's understanding and abilities; but, after the quantities of dissections I have made (and I have many hundred barks drawn by me, besides the twenty I have taken to pieces) I must be well acquainted with that part, and there certainly is not a single sap-vessel to be found in it, except the nourishing vessels passing in their separate cylinders to the leaves. The specimens I have given on figs. 5 and 6, will serve as a pretty good example of barks in general, as they strongly resemble each other.

Chemical trials too soon.

Specimens of bark.

I must apologize for some repetitions in this letter, having already given a description of each separate part. Now that I come to ally those parts to *each other*, to cement the whole, and to shew in what manner they prove the truth of each individual observation, it will not be always in my power to avoid shewing what has already been read. But I will do it as much as the explaining simply and plainly will permit.

Your obliged Servant,

AGNES IBBETSON.

Cowley Cot,
25th Jan. 1813.

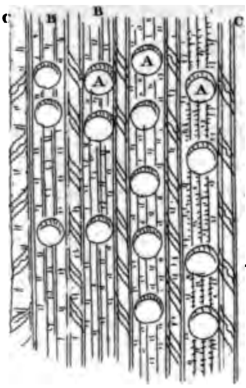
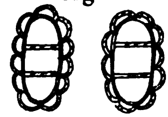


Fig. 1.

Fig. 2.



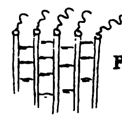
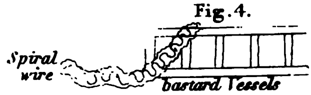


Fig. 3.

Fig. 4.



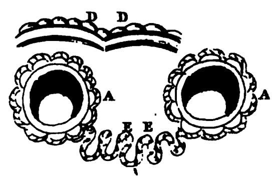


Fig. 5.

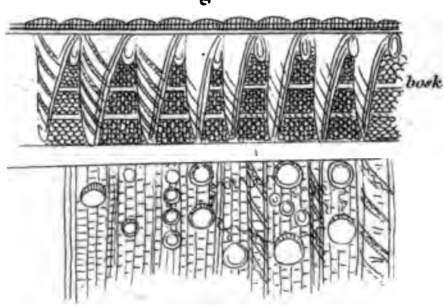
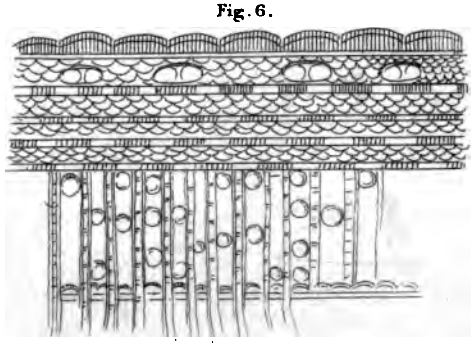


Fig. 6.



IV.

Inquiries relative to the Structure of Wood, the specific Gravity of its solid Parts, and the Quantity of Liquids and elastic Fluids contained in it under various Circumstances; the Quantity of Charcoal to be obtained from it; and the Quantity of heat produced by its Combustion. By COUNT RUMFORD, *F. R. S. Foreign Associate of the Imperial Institute of France, &c.**

(Concluded from p. 335, vol. xxxiv.)

SECTION V.

Of the Quantities of Water attracted from the Atmosphere by Woods of various Species, after being perfectly dried.

IT has been long known that charcoal imbibes the humidity of the atmosphere with considerable eagerness; but I have discovered that dry-wood attracts it with still greater avidity. The following are the details and results of a series of experiments, made last winter, with a view to elucidate this subject.

Having procured thin shavings, about five inches long, and half an inch broad, of nine different species of the woods of our climate; in order more certainly to reduce them to an equal degree of dryness, I began my experiment by boiling them for two hours in water, that they might be thoroughly impregnated with that element.

I then dried them well in a stove, in which they were kept during 24 hours, exposed to a temperature higher than that of boiling water, at about 50° of Fahrenheit's scale.

On taking them out of the stove, they were carefully weighed, being still hot; they were then suffered to remain in the open air for 24 hours, in a large room, whose temperature was uniformly during the day and night at about 45° to 46°, F. This was on the 1st of February, 1812.

The weight of the shavings, on being removed from the stove, thoroughly dried, and after having been exposed to the air of the large room was as follows:

Species of Wood.	Weight. On being withdrawn from the stove.	Weight. After exposure for 24 hours, in a room, at a temperature of 46°F.
Italian poplar...............	3·58 grs.	4·45 grs.
Lime-tree, seasoned, and fit for the joiner's use.......	5 28	6·40
Lime-tree, green-wood.......	5 39	6·47
Beech.....................	7·02	8·62
Birch.....................	4·41	5·47
Fir......................	5·41	6·56
Elm.....................	5·87	7·16
Oak......................	6·46	7·93
Maple....................	4·76	5·85

Hence it appears, that 100 parts of the wood, after 24 hours' exposure in the large room, were composed of dry-wood and water in the following proportions:

100 parts of	Seer-wood.	Water.
Poplar................	80·55......	19·55
Lime-tree, seasoned......	82·50......	17·50
———, green.........	83·31......	16·69
Beech.................	81·44......	18·56
Birch.................	80·62......	19·38
Fir...................	82·47......	17·53
Elm...................	81·80......	17·20
Oak...................	83·36......	16·64
Maple.................	81·37......	18·63

I suffered all these woods to remain in the large room during eight days, but their weight was very little augmented; and as often as the temperature of the air of the room was raised above 46° F. they lost weight. So that the above may be considered as their habitual state of dryness during the winter, in our climate.

To ascertain the quantity of moisture habitually retained by these woods in the summer, I made the following experiments.

Thin shavings of the species of woods mentioned below, half an inch broad, were thoroughly dried in the stove, and then exposed for twenty-four hours, in a room with a northern aspect

aspect, whose temperature was tolerably uniform at 62° F. The following are the results : Quantities of water, &c. in wood.

Species of Wood.	Weight. When dry.	Weight. At the accustomed state of humidity in the air, at 62° F.	In 100 parts of wood were found: Seer wood.	In 100 parts of wood were found: Water.
	Grammes.	Grammes.	Parts.	Parts.
Elm, the core.....	10·53	11·55	91·185	8·815
——, the sap-wood.	11·99	13·15	91·197	8·803
Oak, seasoned, & fit for the joiner's use.	13·70	15·05	91·030	8·970
——felled 6th Sept.	12·45	13·70	90·667	9·333
Lime, seasoned.....	7·27	7·80	93·205	6·795
——, when growing	6·75	7·30	92·466	7·534
——, the root.....	9·96	10·80	92·222	7·778
Elm, seasoned.....	9·25	10·80	91·133	8·867
Italian poplar......	7·50	8·00	93·750	6·250

With a view to ascertain the habitual state of the dryness of woods in autumn, I carefully preserved these same shavings till the 3d of November, in a northern chamber, not inhabited ; at which period its temperature had stood for several days at 52° F. with little variation. I then weighed the shavings, and from their weight calculated the quantity of water contained in them.

The following table, containing the results of all these experiments, displays, in a familiar and satisfactory manner, the customary state of the woods, in different seasons, in our climate.

Species of Woods.	100 parts in weight of wood, cut into thin shavings and exposed to the air, contained water. In summer, at a temperature of 62° F.	In autumn, at a temperature of 52° F.	In winter, at a temperature of 45° F.
	Parts.	Parts.	Parts.
Poplar.........	6·25	11·35	19·55
Lime...........	7·78	11·74	17·50
Oak...........	8·97	12·46	16·64
Elm...........	8·86	11·12	17·20

Quantities of moisture and volatile matter in wood.

From a comparison of these results it appears, that these woods, when exposed to the air at a temperature of 45° F. contain twice the quantity of water, that they do when the temperature of the air is at 60° F. But it is necessary that the wood be cut into very thin shavings, to enable it to become suddenly in equilibrio with the air, conformably to its quality of an hygrometric body; otherwise the state of the air may change, and that very frequently, before its humidity or dryness can have had sufficient opportunity to produce all its effect upon the wood.

To discover what is termed *the medium dryness* of any species of wood, in our climate, it is requisite that we be acquainted with the quantity of water contained in the wood, every day of the year, and even in every hour and every minute, which is obviously impossible: but there is another method to be pursued in this enquiry, much less laborious, and which will lead to results as satisfactory as the nature of the subject will admit.

As a very large piece of wood, a large beam for instance, dries so very gradually in the air, as not to attain a state of perfect dryness in less than 50 or 60 years, it is sufficient to examine the interior of such a beam, after having been sheltered for 80 or 100 years from the rain, to discover the state of such part of the wood as may still be considered sound.

In pulling down old houses, we meet with beams proper for the present enquiry.

An old castle in my neighbourhood being pulled down, I had an opportunity of examining the interior of a large oaken beam, which had, without doubt, been there more than 150 years, and as it formed part of the timbers of the edifice, had been secured from the rains.

A piece of this wood, in a high state of preservation, after it had been planed by the workman, was accurately weighed, and then plunged into water, to ascertain its specific gravity. It weighed 75·05 grammes, and displaced 110 grammes of water, at the temperature of 61° F.; its specific gravity, therefore, was 68227, and a cubic inch weighed 13·33 grammes.

Forty shavings of this wood weighed 11·4 grammes, which were reduced to 10·2 grammes, when they had been thoroughly dried in the stove.

Hence

Hence we may conclude, that a cubic inch of this old wood was composed of

Quantities of moisture and volatile matter in wood.

Ligneous parts.............	0·39794 cubic inch.
Water.....................	0·07186
Air.......................	0·53020
	1·00000

We may also conclude from these results, that the wood of the centre of a large oaken post, though kept for ages out of the reach of the rain, can never contain, in our climate, less than 10 per cent. of its weight in water; and that a cubic inch of such wood contains more than half a cubic inch of air.

The *yearly medium temperature* at Paris is about $54\frac{1}{2}$° F.: now as we have just seen that the habitual state of dryness in woods at the temperature of 52° F. is such as to give about 11 per cent. of water for 100 parts of wood; we must not be surprised at finding 10 per cent. of water in the interior of a large beam, after it had been sheltered from the rain during 150 years.

To ascertain whether the property of wood to attract moisture from the atmosphere was augmented or diminished by the beginning of carbonisation, I made the following experiments.

Fourteen grammes of ash shavings, after being highly dried on a marble slab over a chafing dish, were exposed to the air, in the month of February, in a large room, whose temperature was about 20° F. and in 15 hours they had gained 1·65 grammes in weight.

Fourteen grammes of the same sort of shavings having been first scorched in the stove till they had assumed a brown colour, were at the same time dried over the chafing-dish, and exposed with the others to the cold air, for the same length of time; but they gained in weight only 1·01 grammes, while those which had not been scorched, as already stated, had gained 1·65 grs.

Fourteen grammes of the shavings of lime-wood, in their natural state, and 14 grammes of the same kind of shavings, after they had been violently scorched in the stove, were dried together over the chafing-dish, and then exposed in the open air, at the temperature of 40° F. for 15 hours. The shavings in

Moisture, &c. in wood.

in their natural state gained 1·33 grammes in weight; while those that had been scorched gained only 0·7 grammes.

A similar experiment, upon shavings of the cherry tree, some in their natural state, and others scorched, was productive of the same result.

Whence we conclude, that wood in its natural state attracts the moisture of the air more copiously than it does after having been subjected to the first degree of carbonisation.

From similar experiments upon wood and charcoal, I find that dry wood attracts humidity more powerfully than dry charcoal.

It would be worth ascertaining, whether wood is not also more powerfully attractive of gas than charcoal; but as I have not time to enter upon this particular inquiry, I can only recommend it to those whose inclinations may lead that way. Leaving, therefore, this subject untouched, I must, without any farther circumerration, pursue the original object I had in view in these disquisitions upon wood, *viz.* to endeavour to become acquainted with those inflammable substances, which burn on setting fire to a piece of wood under a calorimeter.

SECTION VI.

Of the Quantities of Charcoal to be obtained from different Kinds of Wood.

Production of charcoal.

Having discovered that pieces of wood, more or less thick, may be perfectly carbonised in glass vases, with thin tops, closely covered, and exposed for two or three days to a moderate heat in a stove, I adopted this method in all my experiments on the carbonisation of wood.

The glass vases which I make use of, are what the chemists call *proofs*, with feet: they are small cylindric vessels, about an inch and a half in diameter, and six inches in height; the covers consist of glass plates, about two inches in diameter, and from two to three lines in thickness, neatly ground with very fine emery, well diluted with water, on a large glass slab; and the edges of the vases being ground with the same exactness, they became hermetically closed by the covers, so as to preclude every access of the air, especially if the edges of the vases, and the whole surface of the covers, be well rubbed with black-lead.

The elastic fluids, in escaping from the interior of the vases, occasionally

occasionally raise the cover for a moment, on one side, even when surmounted by a considerable weight; but as it is only raised a very little, and falls again immediately, the vase is never open more than an instant at a time, and then not so as to admit the obtrusion of any extraneous matter. Preparation of charcoal from wood, &c.

When one of these vases is put into the stove, it is placed upon a square tile, or half brick, of burnt earth, and another of the same kind is also laid upon the cover to keep it steady.

During the carbonisation of the wood, the interior of the vase is always clouded, assuming a very deep blackish yellow colour; and during the operation, a strong smell of soot, or of pyroligneous acid, issues from the stove; which is even insupportable at the commencement, if it be too nearly approached, as well as on withdrawing the vases from the stove, if the covers be removed without due precaution.

There is, therefore, a *decomposition* during the carbonisation of wood, and a formation of pyroligneous acid. This fact has been long known; but in some of my experiments, and particularly in those made upon fir, with a very moderate fire, I obtained a product, which, upon a very exact scrutiny, appeared to me to be *bitumen*.

This product had been condensed upon the glass cover, whence it had afterwards run in large drops upon the vertical surface of the side of the vase. It was hard and brittle, of a dark yellow colour; it was not affected by boiling water, nor by boiling alcohol; but was gradually dissolved by sulphuric ether.

It would be superflous here to enter upon the detail of all my experiments relative to the carbonisation of wood. As the process I have employed cannot now but be well known, after what I have said in this memoir, and in the one that I had the honour to present to the class on the 30th of December, in last year*, I shall here only give the results of those experiments.

The six following, made with different species of wood, were so uniformly alike in their results, that I was much surprised.

One hundred parts (10 grammes) of the six following kinds of wood, in thin shavings, and thoroughly dried, were carbonised at one time in the stove, in glass vases, well closed with flat

* Inserted in this Journal, Vol. XXXII. p. 100.

glass

Charcoal from various kinds of wood.

glass covers. As the heat was managed with great care, in order to determine with precision, from the weight of the vases, the moment when the operation was finished; the experiment occupied four days, and as many nights. When the vases with their contents ceased to lose weight, the process was stopped, and the charcoal was weighed while still hot.

The following were the results.

100 parts in weight of dry wood, gave in dry charcoal.	Poplar.....43·57 parts.
	Lime......43·59
	Fir.......44·18
	Maple....42·23
	Elm.......43·27
	Oak.......43·00

The medium term of the results of these 6 experiments gives 43·33 parts of charcoal in 100 parts of dry wood; and as they were made with woods differing considerably in their apparent weight, their hardness, and other distinctive physical characters, we may conclude from the great similarity of the results of these experiments, that none of the circumstances, from which the woods derive their particular characters, have any material influence upon the quantities of charcoal they are capable of yielding; and hence we may deduce that the ligneous substance, or seer-wood, if not the same in all, is at least composed of indentical substances.

There is still, however, a very interesting question remaining for discussion, *viz.* Is the seer-wood, charcoal?

To elucidate this question, I began by examining whether charcoal had the same specific gravity as seer-wood.

I, therefore, reduced some common oak charcoal, which appeared to be well manufactured, into pieces about the size of small peas, and then boiled them in a pretty good quantity of Seine water, previously well filtered: the pores of the charcoal were speedily so completely filled with this liquid, that becoming heavier than the water, in equal bulk, it precipitated itself to the bottom of the vessel, and there remained.

On removing the vessel from the fire, the water was suffered to cool to the temperature of 60° F.; and then the charcoal, while still submersed, was put into the small glass vase of the hydrostatic

hydrostatic balance, and weighed. Its weight in the water, at the temperature of 60° F. was 2·44 grammes

Inquiries respecting charcoal, &c.

When the charcoal was taken out of the water, it was put into a cylindrical glass vessel 1⅓ inch in diameter, and 6 inches in height, in which it was thoroughly dried in the stove at a temperature of about 265° F.

After it had been six hours in the stove, it was taken out, and weighed, while still hot, and found to be equal to 6·7 grammes; therefore its specific gravity was 157273.

We have before shewn, that the specific gravity of the solid parts of oak, in the state of seer-wood, is 153440.

This is certainly very similar to that of charcoal made of the same kind of wood; but we have not yet proved seer-wood to be charcoal: on the contrary, we have just seen, that it requires 100 parts of seer-wood to obtain 43·33 parts of dry charcoal.

Neither is seer-wood simply an hydrure of dry wood, as we shall see in the sequel.

It should seem that the fabric of a plant, which may perhaps be nothing but pure charcoal, is always covered with a substance analogous to the flesh which conceals the skeleton of an animal. This vegetable flesh does not exist in considerable masses; for as the plant is not under the necessity of moving from one place to another, in search of nourishment, it has no need either of flexible joints in its skeleton, nor of muscles capable of exerting a great force; and it probably arises from the circumstances of the skeleton and the flesh being very intimately blended together, that they are not discriminated and distinguished from each other.

I consider seer-wood as the skeleton of the plant, with the flesh, though quite dried, still adhering to it: and as we have seen that there are 43·33 parts of charcoal in 100 parts of seer-wood, I should say that 100 parts of seer-wood are composed of

Charcoal..............	43·33 parts.
Vegetable flesh, dried...	56·67
Making together......	100·00 parts.

The beautiful analyses of Messrs. Gay Lussac and Thénard, have shewn us that seer-wood is composed of carbon, hydrogen, and oxigen; and that two different species of wood analysed by

Quantity of charcoal obtained from wood.

by them (the beech and the oak) were composed of these three elements in nearly equal proportions. They also discovered that the oxigen and hydrogen in these woods are in the requisite proportions for the formation of water: wherefore they concluded that carbon was the only combustible substance contained in wood.

It will appear in the sequel, how well the results of these ingenious inquiries accord with those of my experiments.

But first, I shall examine what quantity of charcoal it is possible to obtain from different species of woods, under various degrees of dryness, pursuing the method already adopted in my experiments.

From the mode in which charcoal is ordinarily made, a very considered portion is lost, and improvidently burned during the operation.

As it appears to be clearly proved, by the results of the six experiments above related, that the quantity of charcoal to be obtained from any given quantity of wood, is invariably in proportion to the quantity of dry ligneous substance contained in the wood, the inquiry into the quantities of charcoal to be produced from different species of woods, at various degrees of dryness, becomes limited to that of the quantities of wood absolutely dry, contained in the woods in question.

It has been shewn that 100 parts in weight of oak, thoroughly dried, gives 43 parts of charcoal.

We have likewise seen, that 100 parts of oak as dry as it can be made in summer, at the temperature of 62° F. contains only 91 parts of seer-wood, and, consequently, that 100 parts of such wood would furnish only 39·13 parts of charcoal.

From the results of an experiment, of which I have given an account in this memoir, it appears, that 100 parts of oak, in the state wherein it is found when exposed to the winter's air, at the temperature of 46° F. contain only 83·36 parts of seer-wood; consequently, 100 parts of such wood would yield no more than 35·84 parts of charcoal.

From the examination we have made of the oak, in that state in which it is deemed fit for burning, we have found that 100 parts of this kind of wood contain only 76 parts of absolutely dry wood; whence we conclude, that 100 parts of such wood would produce 32·68 parts of charcoal.

It

It has been shewn that 100 parts of an oak felled on the 6th of September, while in a growing state, contained only 62·56 parts of seer-wood, and that consequently 100 parts of such wood would yield only 2·69 parts of charcoal. Quantity of charcoal obtained from wood.

In making these calculations, no account has been taken of the quantity of wood, or other combustible, burnt in order to heat the closed vessel in which the wood was carbonised, pursuant to the process here adopted. But it may be remarked, that such quantity will be increased or diminished according to the construction of the furnace, and the arrangement of the other parts of the apparatus; and it will always be too considerable to be omitted in the list of expences.

As M. Proust obtained only 19 or 20 parts of charcoal in 100 of oak, it is probable that some waste occurred in the process; but as it is certain, that in the carbonisation of wood, some loss will happen; so in the ordinary method of making charcoal, there is always a considerable reduction of the quantity that ought to be produced, arising from the quantity of wood consumed, either wholly or in part, to obtain heat sufficient to char the portion of wood that is reduced to a coal.

Messrs. Gay Lussac and Thenard found from 52 to 53 parts of carbon in 100 of seer-wood; but 100 parts of seer-wood yielded me only 43 parts of charcoal: this difference, however, it is easy to explain, as will be seen in the sequel. Combustion of charcoal.

Section VII.

Of the Quantities of Heat developed in the Combustion of different Species of Wood.

Many persons have already endeavoured to determine the relative quantities of heat furnished by wood and charcoal in their combustion; but the results of their inquiries have not been satisfactory. Their apparatus has been too imperfect, not to leave vast incertitude in the conclusions drawn from their investigations. Indeed, the subject is so intricate in itself, that with the best instruments, the utmost care is requisite, lest, after much labour, the inquirer should be forced to content himself with approximations instead of accurate results and valuations strictly determined.

All woods contain much moisture, even when apparently very

Manner of the experiments for ascertaining the heat from charcoal.

very dry; and as the persons alluded to have neglected to determine the quantities of absolutely dry wood burned by them, much uncertainty prevails in the results of all their experiments.

Another source of uncertainty lies in the great quantity of heat suffered to escape with the smoke and other products of the combustion.

As the calorimeter used in my experiments, has been described in a memoir which I had the honour to present to the class on the 24th of February, 1812, it is unnecessary here to resume that subject: suffice it to explain, in a few words, the various precautions I adopted in burning wood under the calorimeter.

I picked out the woods intended for the experiments, from a joiner's workshop, and they all appeared to be quite dry; I had them formed into small boards, six inches in length, and half an inch thick: from these boards I had some shavings planed off, about 1-10th of a line thick, half an inch broad, and 6 inches in length.

When these shavings were sufficiently dry, they were burned, one by one, under the mouth of the calorimeter; and I took care to hold them, by means of a small pair of nippers, so as to make them burn with a brisk flame, and without the least smoke, or smell, or calculable residuum in ashes.

The following is the method I pursued in making these experiments.

The calorimeter, filled with water at a temperature of about 5° of Fahrenheit's thermometer, lower than that of the apartment in which the experiments were made, was placed upon its stand at the height of about 18 inches above the table on which the apparatus was laid.

The extremity of the calorimeter, containing the opening, which I call its mouth, projects about 4 inches beyond the edge of the stand, so as easily to admit the point of the flame from the small piece of burning wood; and the height of the stand is so adjusted, that the operator may rest both his elbows on the table, while his hands sustain the fragment of wood to be burned.

Near the calorimeter stands a small lamp, by which the pieces of wood, or rather shavings, may, without loss of time, be set on fire, and burned in succession; and care is taken to have always

in

in the hand a sufficient quantity of the shavings, of a known weight. Heat from charcoal.

The very small portions of the shavings which remain between the nippers are carefully preserved and weighed, at the close of the experiments, to determine precisely how much of the wood has been consumed.

An assistant keeps his eye constantly on the thermometer attached to the apparatus, and announces the moment when the water in the calorimeter has attained a temperature as much higher than that of the room, as it was below it at the beginning of the operation; and the flame from the piece of wood then burning is immediately blown out.

The remains of the shaving is laid aside, to be afterwards weighed with the other fragments.

The water in the calorimeter was then stirred, by shaking it, taking care to hold the instrument by its wooden frame, and the temperature of the water was minutely observed and set down in a register.

An experiment of this kind usually occupies about 10 or 12 minutes, according to the nature of the wood, and the number of degrees to which the temperature of the calorimeter is raised.

I made choice of the birch for my first experiments, because the texture of its wood is very firm and even, and burns with a very regular flame.

To give the details and their results in few words, I have placed them together in the subjoined table.

The calorimeter, with the water it contained, was equal in capacity, as to heat, to 2781 grammes of water.

Heat

Results of combustion as to the heat.

Heat developed in the Combustion of Birch-wood.

	No. of the experiments.	Quantity of wood consumed.	Heat communicated to the colorimeter.	Result, with the heat develop. in combustion of 1lb. of combustible. lb. of water heated at 1° of Fahrenheit's thermometer.	lb. of water at the temperature of melting ice, thrown into ebullition.
		gram.	Deg.		
Fire-wood, 2 years old...	1	5·	10½	5875	32·445
	2	4·	8⅓		32·841
Shavings dried in the air..	3	4·55	10¾	6261	34·805
	4	4·54	10¼		34·881
Shavings highly dried over a chafing-dish..	5	3·97	10	7002	38·916
	6	2·58	6⅓		38·925
	7	4·97	1 ⅓		38·858
Shavings highly dried and scorched in a stove...	8	5·07	10¼	5614	31·325
	9	5·10	10¼		31·052
Shavings scorched, but not to so high a degree....	10	4·89	10½	5971	33·174

Remarks.

On comparing the results of these six experiments, all made with the same kind of wood, in their shavings, it will appear that the drier the wood, the greater was the quantity of heat produced from a given weight of shavings. But I found, in taking account of the quantities of moisture contained in the woods, the quantities of heat were always sensibly proportional to those of the dry wood burned; with the exception, however, of the three latter experiments, which were made with wood highly dried for 24 hours in a stove, and which gave several indications, by no means equivocal, of the beginning of a decomposition.

The shavings most scorched in the stove, gave less heat than those which had been less scorched; the two sorts being taken in equal weights.

In all these experiments more or less water dripped from the worm; a certain proof that some hydrogen had been burned: this fact

fact I was very desirous to verify, on account of its great importance to science.

Combustion of wood and of charcoal.

It is not, therefore, mere carbon which furnished all the heat developed in the combustion of woods : of this important fact we shall shortly have an additional proof.

As the great quantity of azote carried along with the products of the combustion, and which, after having passed through the worm, was lost in the atmosphere, also, without doubt, took with it a little more moisture than it had brought into the apparatus; a calculation of the quantity of water formed in the combustion of wood, grounded only on that found in the worm, would be erroneous: though there was always considerably more than necessary to demonstrate that water had been formed.

Before we close this paper, we shall point out a mode whereby the quantity of water thus formed may be estimated, even to such a degree of precision as to leave nothing more to be desired. But it is first necessary to determine the quantity of heat developed in the combustion of the carbon found in this wood, and which was totally consumed.

Although our experiments on the carbonisation of wood, in close vessels, by a moderate fire, leave no doubt as to the quantities of charcoal which the woods therein employed were capable of producing; still the knowledge of this fact is not alone sufficient to enable us to determine the quantity of carbon contained in the wood.

As 100 parts of wood are required for 43 of charcoal, it is evident that the seer-wood is at least partially decomposed, when the charcoal is produced in the process of carbonisation; that is to say, when the skeleton of the wood is deprived of its flesh, and left naked; and it is well known that a great quantity of pyroligneous acid is formed in the carbonisation of wood, and this acid contains carbon.

From the process employed by Messrs. Gay Lussac and Thenard, in their learned analysis, there can be no doubt that they discovered, and kept an account of all the carbon found in the woods analysed by them; and as there was no pyroligneous acid formed in my experiments, when the wood was totally consumed without either smoke or smell, it is manifest that in this case, all the carbon contained in the wood was burned.

According

Combustion of wood and its charcoal.

According to the analyses of Messrs. Gay Lussec and Thenard, 100 parts of oak, perfectly dry, contain 52·54 parts of carbon; and 100 parts of beech contain 51·45.

Now as it seems to me extremely probable that the dry ligneous substance is palpably the same in all woods; I shall take the medium term of the results of these two analyses, and consider it as an indubitable fact, that 100 parts of perfectly dry wood contain 52 parts of carbon.

Therefore, as 100 parts of seer-wood furnished me with only 43 of charcoal; we must conclude, if dry charcoal be considered as carbon, that of the 52 parts of carbon contained in 100 parts of seer-wood, 9 are taken up in the composition of the pyroligneous acid formed in the carbonisation of the wood. Which 19 parts make more than 17 per cent. of all the carbon contained in the wood.

Though charcoal should not be purely carbon, we must, nevertheless, admit that there is still a much greater proportion of carbon employed in the formation of that acid, or of other substances which fly off into the atmosphere during the process of the carbonisation of the wood.

In pursuing inquiries in natural philosophy, the first object that demands attention, is to keep an accurate account of weights; and so long as we proceed with the balance in hand, there is little hazard of being misled.

And here, before I proceed farther in the inquiry into the sources of the heat developed during the combustion of wood, I shall exhibit a general table of the details and result of 43 experiments, made upon 11 different kinds of the woods of our climate. As I shall have occasion to refer to some of these experiments, for the establishment of facts, it is requisite that they should first be known.

All these experiments having been made and registered long before I began the calculations ultimately adopted for the elucidation of their results; I have not hesitated to rely on them. And farther, as they were made with all possible care, and with instruments, to me apparently perfect; I can answer for their accuracy.

New experiments ever bear a certain value; all the knowledge which constitutes the imperishable riches of mankind, consists only of accurate statements of well-conducted experiments. Happy they who have the good fortune of contributing something to the general stock!

Heat

Heat developed in the Combustion of various Species of Woods.

Species of Wood.	*Quality.*	*Number of the Experiments.*	*Quantity of wood burnt.*	*Heat.* Communicated to the calorimeter, whose capacity was equal to 2781 grammes of water.	*Results.* Quantity of water, at the thawing temperature, boiled by the heat developed in the combustion of 1lb. of combustible.
		No.	gr.	Deg.	lb.
Lime - -	Joiner's dry-wood, 4 years old..................	11	4·52	10⅛	34·609
Ditto - -	Ditto..................	12	4·55	10¼	34·605
Ditto - -	Same kind, highly dried over a chafing-dish..	13	4·06	10¼	39·605
Ditto - -	Ditto....	14	3·80	10	40·658
Ditto - -	Same kind, rather less dried.....................	15	5·57	14	38·833
Beech - -	Joiner's dry wood, 4 or 5 years old...........	16	4·74	10¾	33·817
Ditto - -	Ditto......	17	4·72	10¼	33·752
Ditto - -	Same kind, highly dried over a chafing-dish..	18	5·07	12	36·334
Ditto - -	Ditto.....	19	4·43	10⅜	36·184
Elm -- -	Joiner's wood, rather moist...	20	6·34	11½	27·147
Ditto - -	Joiner's dry-wood, 4 or 5 years old...	21	5·28	10⅜	30·359
Ditto - -	Ditto	22	5·45	10⅓	30·051
Ditto - -	Same kind, highly dried over a chafing-dish..	23	4·70	10½	34·515
Ditto - -	Ditto..................	24	5·28	11½	33·651
Ditto - -	Same kind, dried and scorched in the stove.	25	4·	8	30·900
Oak - -	Common fire-wood, in moderate shavings...	26	4·83	8	25·590
Ditto - -	Same kind, in thicker shavings; leaving a residuum of charcoal..........................	27	6·40	10¼	24·748
Ditto - -	Ditto, in thin shavings.	28	6·14	10½	26·272
Ditto - -	Ditto, thin shavings, well dried in the air....	29	7·22	13	29·210
Ditto - -	Joiner's wood, very dry, in thin shavings. ..	30	5·30	10¼	29·680
Ditto - -	Ditto...	31	5·33	10¼	19·796
Ditto - -	Thick shavings, leaving 0·92 grains. of charcoal	32	6·48	11	26·227
Ash - -	Joiner's common dry wood........................	33	5·29	10½	30·666
Ditto - -	Same kind, shavings dried in the air..	34	3·78	8¼	33·720
Ditto - -	Same kind, highly dried over a chafing-dish.	35	5·23	12	35·449
Maple - -	Seasoned wood, highly dried over a chafing-dish..	36	3·85	9	36·117
Service- -	Ditto....... . ...ditto..	37	4·49	10½	36·150
Ditto - -	Same kind, scorched in a stove.................	38	4·50	9	32·337
Cherry -	Joiner's dry wood.......	39	4·75	10¼	33·339
Ditto - -	Same kind, highly dried over a chafing-dish	40	4·36	10½	36·904
Ditto - -	Same kind, scorched in a stove......	41	5·	11¼	34·763
Fir - - -	Joiner's common dry wood.........	42	5·35	10½	30·322
Ditto - -	Shavings, well dried in the air..	43	4·09	9	34·
Ditto - -	Highly dried over a chafing-dish..	44	3·72	9	37·379
Ditto - -	Dried and scorched in a stove........	45	4·40	9½	33·358
Ditto - -	Thick shavings, leaving much charcoal.	46	4·51	6½	28·695
Poplar -	Joiner's common dry wood....	47	4·13	9¼	34·601
Ditto - -	Same kind, highly dried over a chafing-dish.	48	3·95	9½	37·161
Hornbeam -	Joiner's dry wood·· ··································	49	4·98	10¼	31·800
Ditto - -	Ditto..... ··	50	5·01	10¼	31·609
Oak -- -	Dried to {81·4 wood / 19·6 water} imperfectly burned, leaving a residuum of charcoal, in the combustion of... .... {grams. 0·81}	51	6·14	10½	26·421
	{0·73}	52	4·83	6	25·591
	{0·94}	53	6·71	11	25·917

These

Existence and developement of charcoal in wood.

These experiments might lead to a great number of observations; but I shall endeavour to reduce them to the exposition of a few simple facts which they present

One fact, certainly very curious, and of the first importance to the knowledge of the vegetable economy, appears to be well established, viz. that the skeleton of trees is pure charcoal, and that it exists in a perfect state in wood.

If this charcoal did not exist, perfectly formed, in wood, it could not possibly preserve its form, while its envelope of vegetable flesh is destroyed by the fire in the process of carbonisation.

As the vegetable flesh contains hydrogen as well as carbon, it is more inflammable than charcoal, and is consumed at a lower temperature; and, by proper management of the fire, it may be totally destroyed without the inclosed skeleton of charcoal being injured.

Some months ago I presented the class with a small sprig of charcoal, produced from a piece of oak partially burned under my calorimeter. It was nearly all the charcoal contained in the piece. All the coat or flesh of the wood burned with a brisk flame, and the skeleton of the wood had got red, but the heat was not sufficient to consume it.

The charcoal-maker seldom does more than burn the flesh of the wood, and leaves the skeleton of charcoal naked.

The dry vegetable flesh produces more heat in its combustion than an equal weight of dry charcoal.

Shavings scorched in the stove, by a great heat, yield less heat in their combustion, than shavings of the same kind of wood, whose vegetable flesh has not been touched. *See experiments, No.* 5, 6, 7, 8, 9, 10, 25, 38, 41, 45.

In tables of experiments, similar to those registered in the preceding table, it is scarcely possible to have errors on the greater side; but they may easily enough happen on the lesser. We may, therefore, place the more confidence in those wherein the quantities of heat manifested have been the greatest.

In the experiments, No. 13 and 14, the wood of the lime-tree, dried over a chafing-dish, was productive of more heat than any other wood that I examined.

The result, it will be seen, was for 1lb. of this wood burned in experiment,

No.

	No. 13	- -	39·605lb. of water, heated 180° F.	
And in	No. 14	- -	40·658	Do.
	Medium	- -	40·1315	Do.

Combustion of wood and of charcoal.

In order accurately to ascertain how much water this wood contained, I dried thoroughly in the stove a parcel of shavings, which had been previously dried over the chafing dish, and found that it still retained 6·977 per cent. of water.

Therefore we may conclude, that 1lb. of this wood contains only 0 93023lb. of seer-wood.

Now, if 0·93023lb. of seer-wood will heat 40·1315lb. of water to 180° F., 1lb. of the same wood ought to heat 43·141 lb.; and I therefore take this quantity of water heated to 180° F. as the standard of the heat developed in the combustion of 1lb. of wood perfectly dried.

Many persons have endeavoured to account for the heat manifested in the combustion of wood, by attributing it altogether to the charcoal contained in the wood burned.

This hypothesis we have now to examine.

It has been seen, that 100 parts of the wood of the lime-tree, perfectly dried, yielded 43·59 parts of charcoal; consequently 1lb. of this wood, thoroughly dried, can contain only 0·4359lb. of charcoal.

According to the results of Crawford's experiments, which we have found to be very accurate, 1lb. of charcoal furnishes in its combustion only the necessary heat for raising 57·608lb. of water to 180° F.; therefore the charcoal contained in 1lb. of dry lime-wood, equal to 0·4359lb. can furnish in its combustion no more heat than is necessary to raise 25·111lb. of water to 180°; but as the experiment has given 43·141lb., there must certainly have been some other substance burned besides the charcoal, and which could have been none other than hydrogen.

Before we determine the quantity of hydrogen consumed, it is essential to ascertain how much heat has been furnished, not merely by the charcoal itself, but by the charcoal and the carbon contained in the wood; for it is very certain, that all the carbon was burned, since no pyroligneous acid was formed.

Combustion of wood and of charcoal. According to the analyses of Messrs. Gay Lussac and Thenard, 1lb. of dry wood contains 0·52lb. of carbon.

If we adopt Crawford's estimate, we shall find, that the combustion of 0·52lb. of carbon ought to furnish heat sufficient to raise 29·956lb. of water to 180° F.

Deducting this quantity of water from that given by the experiment, viz. 43·141lb., we shall have 13·185lb. as the measure of the heat produced by the combustion of the hydrogen consumed in the experiment.

From the results of this inquiry we may conclude, that if the heat manifested in the combustion of wood, rather more than two-thirds are produced by the combustion of the carbon, and a little less than one-third by the hydrogen consumed.

These data supply us with an easy method of determining the quota of free and combustible hydrogen contained in seer-wood.

According to Crawford's estimate, which we have followed all along, 1lb. of hydrogen yields in its combustion heat sufficient to raise 410lb. of water to 180° F.; therefore, the 13·185lb. heated to 180° in the experiment in question, must have required 0·035158lb. of hydrogen, which is consequently the amount of free and combustible hydrogen contained in 1lb. of seer-wood.

Assuming the medium term of the results of the two analyses of dry wood, made by Messrs. Gay Lussac and Thenard, 1lb. of seer-wood would be composed of

Carbon	0·52lb.
Hydrogen and oxygen, in the necessary proportions for forming water	0·48
	1·

From the results of my experiments, 1lb. of seer-wood is composed of two distinct substances, viz.

A skeleton of charcoal, weighing	0·43lb.
Vegetable flesh	0·57
	1·

And

Combustion of wood and of charcoal.

And these 0·5715. of vegetable flesh are composed of

Carbon, free and combustible	0·090
Hydrogen, free and combustible	0·035
Hydrogen and oxygen, in the necessary proportions for the formation of water	0·445 ;
	0·570

In making these estimates, I have availed myself of the valuation of the total quantity of carbon contained in seer-wood, given in the analysis of Messrs. Gay Lussac and Thenard; and I have supposed the 43 per cent. of charcoal, which I found contained in seer-wood, to be pure carbon.

Should it ultimately appear, that charcoal is not pure carbon, which is extremely probable, numerous alterations in all these estimates must follow, though the experiments made upon the woods will always retain their value. And I cannot but hope, that they will be frequently repeated, with such variations as may conduce to important discoveries.

It will be a satisfaction to me to know that I have put into the hands of more skilful workmen than myself, some instruments of which they may advantageously avail themselves; and to have pointed out, as well as a little smoothed, a new path, wherein they may walk without danger of being lost.

Section VIII.

Of the Quantity of Heat lost in the Carbonisation of Wood.

In making charcoal, a considerable quantity of heat is dissipated and lost in the air; whence it is evident, that the same amount of heat cannot be obtained from burning a given quantity of charcoal as would be furnished by the combustion of the wood of which it is formed.

We can now determine, with great precision, the loss of heat which is inevitable in making charcoal, even when all possible precautions have been taken; as well as that which happens every day in the process employed by the charcoal-maker.

Combustion of wood and of charcoal.

As the combustion of 1lb. of charcoal, perfectly dry, yields heat sufficient to boil 56·608lb. of water, at the thawing temperature; and as 1lb. of wood, thoroughly dry, furnishes 43·33lb. of dry charcoal, it follows, that the charcoal produced from 1lb. of dry wood, should furnish in its combustion heat sufficient to boil 24·958lb. of water, at the thawing temperature.

But we have already seen, that the combustion of 1lb. of wood, thoroughly dry, should furnish sufficient heat to boil 43·143lb. of water at the freezing temperature; or, which is the same thing, to raise it to 180° of Fahrenheit's thermometer.

These two numbers (43·143 and 24·954) which express the quantities of heat in question, being in the proportion of 100 to 57·849, it is evident, that the loss of heat *inevitable* in the carbonisation of wood, is upwards of 42 per cent., or exactly 42·151 per cent. of the total quantity that the wood will furnish.

In order to determine the loss of heat which occurs in the forests, by the ordinary process of the charcoal-burner, it is requisite to ascertain the precise product of charcoal from a given quantity of wood, though it is probable that this product is very variable. M. Proust estimates it at 20 per cent. in weight at the highest.

Adopting, for a moment, this estimate, and supposing the carbonised wood in the same state of dryness as what is usually sold for fire-wood; as 100lb. of such wood contains only 0·76lb. of perfectly dry wood, this quantity would furnish in its combustion only the degree of heat necessary to raise 32·043lb. of water to 180° F.

But the 0·20lb. of charcoal produced by the carbonisation of 1lb. of this wood, according to the usual process, can only furnish by combustion a sufficient quantity of heat to raise 11·521lb. of water to 180° F.; and as the numbers 32·043 and 11·521 are nearly in the proportion of 100 to 36, it should seem, that the loss of heat in question is about 64 per cent.

One very important fact, which appears to be well ascertained by the results of this inquiry, is, that all the charcoal produced from the carbonisation of 3lb. of any kind of wood, scarcely

scarcely gives more heat in its combustion than would be furnished by 1lb. of the same sort of wood burned, and in its natural state.

V.

A Mode of producing intense Cold. *By* M. H. B.

To Mr Nicholson.

SIR,

I HOPE you will give early insertion to the following proposal for producing intense cold. The proposal itself is all that I can communicate at present; perhaps I may be able, in a short time, to send you an account of some experiments on this subject.

Diminution of temperature by exhaustion proportioned to difference of density produced.

The diminution of temperature observed during the exhaustion of the receiver of an air-pump, appears to suggest a principle on which the degree of artificial cold may be very much increased. The degree of cold produced in this way appears to be proportionate to the degree and rapidity of the rarefaction of the air contained in the receiver; to increase the quantity of rarifaction, therefore, would be to increase the intensity of the cold. The quantity of rarefaction will be measured by the difference between the density of the air employed, before and after the experiment; the denser the air is before, and the rarer after the experiment, and the greater the rapidity with which this change is effected, the more intense will be the cold induced.

It is proposed to condense the air of a receiver, and then suddenly rarify it.

To generate artificial cold, it is proposed, that a cylinder be filled with air, which is, by means of an accurate piston, to be subjected to very strong pressure. The cylinder and contained air are to be cooled as much as possible by the best pijocific mixture, and in this state the air is to be allowed to escape through an orifice, into a large exhausted receiver. Any substance contained in the cylinder, or exposed to the stream of expanding air in the receiver, will have its temperature very much reduced. And as the air may be compressed to an indefinite

finite degree, it would appear that there would be no limit to the degree of cold which might be produced in this manner.

I am, Sir,

Your obedient Servant,

M. H. B.

Edinburgh, May 17, 1813.

VI.

An explanatory Statement of the Notions or Principles upon which the systematic Arrangement is founded, which was adopted as the Basis of an Essay on Chemical Nomenclature. By Professor J. Berzelius.

(*Continued from p. 47 of the present Volume.*)

Remarkable facts of combustion and theory.

IN order to determine whether this phenomenon of combustion actually consists in an absorption of oxigen or not, I prepared quantities of the stibiates of cobalt and of copper, as well as the stibiates of these same metals. By exposing them to a cherry red heat, I deprived them of their water of crystallization, and when, by a new exposition to this same temperature, they did not lose in their weight, I considered them as entirely deprived of the water of combination. I then weighed them very carefully, and heated them in a small platina crucible (exactly weighed) to incandescence. They took fire with great brilliancy, and the same thing took place in the open crucible, as when it was provided with a cover, which shut very closely, and consequently excluded the air. When the ignition took place in the small crucible, which was closed, it became all at once incandescent, and in the lower part, which was in contact with the ignited mass; so that the interior of the crucible became visible when this phenomenon took place. On making this experiment in an open crucible, at the moment of ignition, there arose a slight smoke, which, in a crucible with a cover, was condensed on the top, and proved to be the stibious acid. I have great reason to suppose, that this smoke owes its origin to some small

small adherent portions of the paper of the filtre, which, notwithstanding all my care to prevent it, were mixed with the powder. The saline mass, on exposure to ignition, instead of increasing in weight, lost from one-fourth to two-thirds in the hundred, which must be attributed to the volatilization of the stibious acid. I repeated each of these experiments several times, and the quantity of sublimate varied constantly between these limits. The stibiate of copper produced no visible smoke, and lost scarcely any thing by the ignition. It sometimes happened, that the ignition was only partial, and then the colour was only altered in those parts which had undergone ignition: but even those parts where the colour was not changed, appeared equally to resist the action of acids. Theoretical considerations.

These experiments prove, then, that the ignition in question cannot be produced by a combination with the oxigen, or with any other body, which did not exist in the previous combination. But what, then, is the cause of it? We are not as yet acquainted with any example where the same bodies may be combined in the same proportions in their different degrees of actual saturation: that is to say, where there are between these two bodies two degrees of what may be called *intimacy of combination*. Such an hypothesis would explain the phenomenon of ignition, as well as the new forces of affinity might be supposed to have been acquired by that means.

In this case it is necessary to imagine, that a fresh portion of negative electricity of one of the constituents, is combined with a correspondent portion of positive electricity of the other, and produces, by this electric discharge, the phenomenon of fire, while the combination, at the same time, arrives at a much higher degree of electro-chemic indifference than before. It is generally known and admitted, that bodies can combine indifferent degrees of intimacy of combination, and in different proportions: such as iron, which is combined much more intimately with the quantity of oxigen, which renders it oxidum ferrosum, than with that which converts the ferreous into the ferric oxide; and we see the same difference of intimacy of combination with different quantities of oxigen in a number of other combustibles; but there is always a consideration of the different proportions of parts. Nevertheless, the experiments I have here cited, appear to indicate, that the same difference of

Theoretical considerations.

of intimacy of combination which we find between different proportions of the same substances, may, under certain circumstances, exist between the same proportions.

The phenomena here related will afford for the theory of fire and of chemical combination, some very important results, which cannot, perhaps, be yet foreseen, and approaches what the celebrated Davy discovered in the decomposition of superoxidum muriaticum* (Euchlorine D.) At present the most probable explanation appears to be, that the stibic and stibious acids can form combinations with several saline bases in two different degrees of intimacy; and both in the same proportions between the acid and the base. The salts possessing all the characteristics of combinations, no chemist can consider them as mechanical mixtures. But when they become heated, they

* Sir H. Davy has found, that the superoxidum muriaticum, is decomposed at a temperature between 35° and 40°, with the phenomena of fire, and produces oxigen gas and gas superoxidum muriatosum. There is, then, in this instance, a chemical separation, accompanied by the same phenomena which take place in all acts of intimate combination. There is, without doubt, a great anomaly which, in the hypothesis of chlorine, will remain an enigma. If, on the contrary, the superoxidum contains, according to the laws of determinate combinations, twice as much oxigen as muriatic acid, it may be supposed, that this second portion of oxigen is therein combined with an extremely weak affinity, which, in their combination, was not capable of producing the phenomenon of fire. The superoxidum muriatosum (oxymuriatic gas) is a combination of muriatic acid with half as much oxigen, which the acid does not contain itself, and the oxigen is found combined in a much more intimate degree; so much so, that it cannot be expelled without a complex affinity. At a temperature rather elevated, the gas superoxidum muriaticum produces the gas superox. muriatosum, by combining the half of its excess of oxigen with the muriatic acid, in a more intimate manner, and producing, for this reason, the phenomenon of fire. The other half of the oxigen, yielding to a stronger affinity, is disengaged, and the gas is expanded. The only difficulty in this explanation is to consider the superoxidum muriaticum as composed of muriatic acid and oxigen, and not as composed of superox. muriatosum and oxigen, which is our usual manner of considering the different degrees of oxidation, and which appears to be correct in a number of circumstances; but which, nevertheless, is not a manner of combination requisite in all cases. In short, I merely quote this explanation as a probability, and as the only one I have as yet been able to contemplate.

produce

produce a phenomenon which indicates, that a fresh combination has taken place. Caloric is disengaged, and as the temperature is already very high, this disengagement of the caloric is sufficient to produce intense ignition. On this occasion the mass neither gains nor loses in weight, but its colour and characters are altered, and the constituents are found to have entered into another state of combination which may be called *more intimate*, because they cannot be destroyed by the affinities of bodies which had the property of decomposing these substances before this change had taken place. We know that a very elevated temperature produces, in several compound bodies, a degree of chemical indifference; for example, the sulphate of alumine, the ferreous sulphate, the muriate of nickel, &c. on being heated to a certain degree, appear to become insoluble in water, and it requires the action of water to be continued a length of time, in order to restore their solubility. Alumine, zircon, the oxides of titanium and of tautalite, &c. lose their solubility in acids, when heated to a red heat; several combinations of the earths and metallic oxides, which are found in the interior of the earth, are not to be decomposed by the strongest acids, notwithstanding the affinity of these acids to the earths, under common circumstances, is infinitely stronger than that of the earths to each other. In all these cases it is necessary, in order to bring them to the ordinary state of combination, to expose these substances to the action of a strong chemical agent, and frequently at an extremely high temperature; for example, to burn them with alkalies, or alkaline earths, or with phosphoric acid, &c. I presuppose, in this instance, the electro-chemic indifference is destroyed in the same manner as the oxide of mercury is decomposed by heat, and that the constituents re-appear separated, and endowed with their original electro-chemic quantities. It may, then, be very probable, that all these phenomena arise from one same cause, and that they are not effective but at a degree of combination more intimate than that which belongs usually to bodies produced by chemical operations in aqueous solutions, or at least where water is present.

Theoretical considerations.

The

The Oxides of Tin.

Oxides of tin. We now come to a metal which has long been known, and, for that reason, has not been examined with the same care as those to which we owe our knowledge in the discoveries of the present day. Notwithstanding I have taken much trouble in order to determine the degrees of the oxidations of tin, as well with regard to their number as to their properties, I cannot venture to hope I have exhausted this subject, but that much still remains to be explained by future experiments.

We find, amongst the oxides of tin, the same general characters as belong to antimony, but with differences, which begin a transition of metals, from electro-negative to electro-positive.

In order to ascertain whether tin possesses a suboxide or not, I endeavoured to examine the black film which is formed on the surface of melted tin. To procure this black substance, I exposed some extremely thin leaves of tin to a very high temperature, taking care not to melt them. They became blackened, and, when it appeared to me, that the leaves were entirely covered, I took them from the fire. On examining them, I found the oxided film so extremely fine, that it could not be separated from the metallic tin. I then exposed the leaves again to a continued heat, but it appeared that the film had prevented the air from coming into contact with the metallic parts. When the leaves were heated to nearly a red heat, they burnt brightly, and left a whitish powder. Not being able to separate the black oxide produced in this manner, I endeavoured to procure the oxide of tin, oxidum stannossum, which the elementary books describe as white, in order to compare it with this black and combustible oxide, and to discover, by that means, if the latter was actually in a degree of oxidation inferior to the stanneous oxide, *oxidum stannossum*. I then dissolved pure tin in concentrated muriatic acid; the solution was precipitated by the carbonate of potash, and I washed the precipitate on a filtre with boiling water. It was perfectly white, and preserved its colour after being dried. Some chemists assert, that the caustic alkali precipitates the stanneous muriate of a grey colour. I have not found it so; the caustic alkali readily produces a

whit

white precipitate, as well as the carbonate; but this precipitate is easily dissolved in a large portion of caustic alkali; and it does not become the least grey, even the moment before its dissolution. Oxides of tin.

The white stanneous oxide, precipitated by the carbonate of kali, dissolved easily, and without disengaging carbonic acid in the acids, and even in the nitric. It is, therefore, not a carbonate. When heated in a small retort, which was half full, it yielded pure water, and left a greyish powder, which was black at the bottom. I filled the retort a second time entirely with the white oxide, and heated it anew. I obtained water, but the oxide which remained was quite black. On being taken out of the retort, and triturated into a fine powder, it had a colour composed of green, brown, and grey. Heated at the point of the flame of a candle, the oxide took fire, and continued to burn like amadon, or tinder fungus, leaving a white greyish powder. It follows from this, that the stanneous oxide is black, and that the white powder precipitated by the alkali was only a hydrate of the oxide. This hydrate is exactly the same as the oxide itself, very combustible, and, when lighted at the point of flame, it continues to burn of itself, though with less brightness than the pure oxide. If the hydrate be carefully washed and mixed with water, and the water heated to ebullition, the hydrate is decomposed, and the black stanneous oxide remains by itself. The phial in which I made this experiment was, by accident, laid aside in a place where it remained four months; and the black oxide was, at the expiration of this time, very little altered; for it had only a slight layer of the white oxide on the surface.

These experiments prove, that there is nothing in the characters of the stanneous oxide, which justifies the conclusion, that the black film produced on melted tin, is a sub-oxide, and, consequently, the existence of this sub-oxide, is yet very problematical.

In order to determine the composition with regard to quantity of the stanneous oxide, it not being possible to be done in a direct manner, I employed the following method:

I melted together, in a glass retort, pure tin and sulphur, and obtained a greyish mass, which was porous, and had a metallic lustre; I then reduced it to powder, and mixed it with half of

its

Oxides of tin. its weight of pure sulphur pulverized. I exposed this to a suddenly elevated temperature, in a glass retort, in order to melt it, and I kept it in a state of fusion, until the sulphur was volatilised. The upper part of the retort contained a small quantity of mosaic gold. The sulphuret of tin had a metallic lustre, of the colour of lead, and the broken part was crystalline, and very brilliant. When dissolved in concentrated muriatic acid, it gave out sulphurated hydrogen gas, which, being passed through a ley of caustic kali, was entirely absorbed by it. Hence it follows, that the sulphur of the sulphuret of tin, is, to the oxigen of the stanneous oxide, as the sulphur in sulphurated hydrogen gas, is to the oxygen in the water. Another part of the sulphuret of tin was treated in a glass phial, with fuming nitric acid, until it was entirely decomposed; the contents of the phial was then carefully poured into a crucible of platina, dried, and at last heated to a red heat, until all the acid was expelled. 100 parts of the sulphuret, produced in this manner, 99·5 parts of the white oxide of tin. From this, it follows, that the oxigen in the white oxide, is nearly equal in quantity to the sulphur of the sulphuret, consequently, the white oxide produced by the nitric acid, contains twice as much oxigen as the stibious oxide. According to the experiments I have already published, the white oxide of tin is composed of 100 parts of metal, and 27·2 p. of oxigen; the stanneous oxide is therefore composed of:

Tin	88·028	100·0
Oxigen	11·972	13·6

White Oxide of Tin.

It is generally admitted, or at least, as far as I know, never disputed, that the oxide of tin, which is found in the volatile combination, known by the name of *spirit of libavius*, is the same as that which is formed when the tin is burned in the air, or oxided by nitric acid. We find it, nevertheless, sometimes observed by chemists, that the latter is insoluble in the acids, when, on the contrary, the first ought to be soluble in them, as it exists in the liquor of libavius. The cause to which that is attributed, has generally been, that the oxide produced by the combustion, or by the nitric acid, was to that in the spirit of libavius, as the ignited zircone to that contained in the salts. The

The following experiments put it beyond doubt, that these two oxides have not the same degree of oxidation. Oxides of tin.

A solution of the spirit of libavius in water, was precipitated by the carbonate of potash, and the precipitate washed with cold water. On pouring cold water a second time upon it, it melted like butter on the filtre, and a liquor which had a milky appearance, passed through the paper, and was precipitated afresh when it fell into the alkaline liquor, which was already filtrated. After the space of 24 hours, it became clear, and was of a pale yellow colour; it had an alkaline taste, notwithstanding, it was precipitated afresh by the addition of alkali. The nitric acid, as well as the muriatic acid, produced a precipitate, which, nevertheless, was dissolved by an addition of acid in excess. The solution made by the muriatic acid, was not altered by ebullition, but that which was made in the nitric acid, formed a jelly a little time before it arrived at the temperature of 100°.

Another portion of this oxide on which cold water had been poured but once, was dried by being pressed between thick folds of blotting paper, and the drying was at last accomplished, by exposing it, in a temperate place, to a current of air. The oxide then took the form of little colourless pieces, some transparent, and resembling pulverized glass. It comported itself with the acids in the following manner:

In *sulphuric acid*, diluted with an equal quantity of water, a great part dissolved, but when a large quantity of acid was used, it was totally dissolved. When the oxide before being perfectly dry was treated with the sulphuric acid, much diluted with water, it lost its transparency, and fell in a powder, and produced a soluble super-sulphate, and an insoluble sub-sulphate.

Nitric Acid has the property of dissolving this oxide until its acid taste is replaced by an astringent one. The saturated nitric solution becomes gradually turbid, and, on being exposed to an elevated temperature, it coagulates, loses its astringent taste, and becomes acid again. I have not been able to perceive, that, on this occasion, any of the nitric oxide gas (nitrous gas) is disengaged, probably because the acid is only reduced to nitrous acid.

Muriatic Acid softens it at first, and at last dissolves it; and the

Oxides of tin. the solution is not altered neither by boiling, nor by the addition of a great quantity of concentrated muriatic acid, nor by that of water.

When I precipitated a solution of the spirit of libavius by the caustic ammonia, the preceding phenomena took place again—that is to say, when I poured water for a second time on the precipitate, it was softened, and began to pass through the paper filter with the water. The lactescent liquor which passed through the paper, was again precipitated by the addition of more ammonia. On being evaporated, it yielded a yellowish jelly, which dissolved in cold, as well as boiling water, and was also soluble in the acids. The oxide containing the alkali, when dried at a moderate temperature, is colourless, transparent, and soluble in the acids. Dried at a more elevated temperature it lost its solubility in acids; probably in consequence of an ulterior oxidation. The same thing took place when it was heated to a red heat: it then became, in proportion as the heat increased, yellow and orange, and lastly a very deep red, like cinnabar; but as it cooled, these colours disappeared in an inverse order, and the oxide only preserved the pale yellow of the citron. The properties which form the distinguishing characters of this degree of oxidation are, to afford a very volatile salt with muriatic acid; and this salt is not decomposed either in boiling water, nor by the addition of concentrated muriatic acid, nor by the salts of gold or copper.

Yellow Oxide of Tin.

The oxide which is obtained by the action of nitric acid upon tin, is white, and preserves, as I have remarked in another place, although it may have been washed in the most careful manner, the property of giving a red colour to turnsole. During the drying, it became nearly transparent, yellow, and hard; on exposure to fire, it lost the water, and, at an elevated temperature, assumed a deep brown colour, which, after refrigeration, remained only a yellow: and on being pulverized, almost entirely disappeared.

A portion of the oxide of tin, obtained by the nitric acid, carefully washed, but not dried, was digested with concentrated muriatic acid in a bottle well stopped. The acid assumed a yellowish colour, but the oxide remained perfectly insoluble. At the expiration of twelve hours, I opened the bottle; some oxymuriatic gas escaped with a slight explosion. I then decanted the acid, and poured a small quantity of water upon the oxide. After the oxide was again deposited, I poured off the water, and added a fresh quantity to the oxide which was instantly dissolved by it. Oxides of tin.

The solution tasted very astringent, was colourless, but not perfectly clear. I mixed a portion of the solution, obtained in this manner, with a portion of concentrated muriatic acid; which caused an abundant precipitation. On the liquor being poured off, the precipitate was again dissolved in water. Another part of the same solution was heated in an earthen vessel over a lamp of spirit of wine. It coagulated first at the bottom and then throughout the whole mass; and in this state it had so much the appearance of albumen, that I should, without hesitation, have taken it for that substance, if I had not known what it was. This mass had now lost its astringent taste, and become acid; and the precipitate was not re-dissolved during the cooling. At this temperature the water had, therefore, precipitated the oxide by separating it from the acid. The precipitate washed and heated afresh with the concentrated muriatic acid, re-produced the same phenomena which I have described.

These experiments prove, then, that the oxide produced by the nitric acid can be combined with the muriatic acid. This combination is soluble in water, but it may be separated from the water by an excess of muriatic acid, and is decomposed by the action of water at an elevated temperature. These properties, therefore distinguish this oxide in a decided manner, as very different from that found in the spirit of libavius.

The resemblance of these two oxides as to their external properties, the facility with which one appears to be converted into the other, without it being possible to discover them by any visible change, and their habitudes with the alkalis being exactly the same, makes this disquisition not very easy, and may give cause to doubt the accuracy of what I have concluded from the experiments here mentioned. Nevertheless, in order to put the subject beyond dispute, I mixed some spirit of libavius with nitric acid, and evaporated the mixture till it was entirely dry, Oxides of tin. dry, and the oxide by the nitric acid remained. I then dissolved pure tin in a mixture of the nitric and muriatic acids, but with a greater proportion of the former. It produced during the time it was dissolved, a white insoluble mass, which, after being filtered on a paper, was dissolved by pure water, and was the muriate of the oxide at a maximum.

The solution of tin distilled in a retort, at first produced water mixed with a little nitric acid, afterwards the spirit of libavius which became more and more concentrated, and at the same time disengaged a small quantity of oxymuriatic gas. It remained at last in the retort in a white powder, which at a red heat did not appear to alter. A small quantity of this powder being taken out of the retort and mixed with water, was entirely dissolved by it. The solution was coagulated by ebullition, and precipitated by an addition of muriatic acid, consequently it was the muriate of the oxide at a maximum, which remained in a fixed state, at a temperature greatly surpassing that required to volatilize the liquor of libavius. The portion of muriate which remained in the retort exposed to ignition, slowly produced the spirit of libavius, and disengaged at the same time oxymuriatic gas; but even after I had exposed in a platina crucible to the highest possible temperature, muriatic acid remained, which I separated by means of the alkali.

This experiment proves, that when tin is dissolved by nitromuriatic acid, it forms itself into two distinct muriates, of which one is deposited in the solution when it begins to contain a considerable quantity; and when the liquor is distilled the spirit of libavius is obtained in the receiver, while the muriate of the oxide at a maximum remains in the retort. This latter is, with some difficulty, decomposed by heat, and produces oxymuriatic gas and the spirit of libavius.

It would be, undoubtedly, a very interesting thing to decide what different influences these two oxides of tin have on colours, for which the nitromuriatic solution of tin is often employed as a mordant.

As it is proved by these experiments that the oxide in the spirit of libavius is not the same as that formed by the nitric acid, and as this latter ought to contain more oxigen than the former, it is evident, that it should contain an intermediate quantity between the stanneous oxide and the oxide produced by by the nitric acid. This intermediate degree can be no other than the multiplication by $1\frac{1}{2}$, and by the numerous examples we already know of this series, it appears that we do not require any special analysis, which at least would be very difficult to effect, in order to determine the composition of the intermediate oxide, as follows : Oxides of tin.

Tin	83·13	100·0
Oxigen	16·87	20·4

The distinctive characters of the oxide at a maximum are the following : Digested with a lixivium of carbonate of alkali, it does not dissolve, but part of the alkali is combined with the oxide ; but when the excess of alkali is taken from it along with the lixivium, the combination of the oxide with the alkali is soluble in pure water, and forms a milky fluid, which, after the deposition of the part not dissolved, is yellow. When this liquor is evaporated, it leaves a yellow jelly, which, in its dry state, is full of cracks, and has some resemblance to amber. It is soluble again in cold water. The liquor here mentioned is precipitated by both alkalis and acids. The sulphuric, nitric, acetic, and oxalic acids in excess do not reduce the precipitated oxide. If this oxide be mixed with concentrated sulphuric acid, it gains in volume, becomes yellow, but does not dissolve. The yellow powder seems to be a combination with the acid ; it is easily decomposed by means of water, which takes up the acid, and leaves the oxide its white colour. This oxide will combine with muriatic acid, and an excess of acid renders it insoluble in water. The neutral combination is easily dissolved by it, but is precipitated by an addition of muriatic acid, and is decomposed at a point of temperature which is not as high as boiling water. The oxide is deposited by coagulating, and the fluid becomes a mass exactly resembling coagulated albumen. The oxide at a maximum, combines with water, and it is then white, but on being exposed to heat, it gave back the water, and became yellow. It produced no alteration in it when exposed to the highest temperature possible, in a covered crucible of Platina. Urged by the flame before a blow pipe, it became white at the part where the point of the flame touched it. Is this a reduction of the intermediate oxide ; and is the white enamel, which is obtained by melting

 the

Oxides of tin. the oxide at a maximum with plumbiferous glass, a combination of the intermediate oxide with the glass?

I ought, lastly, to remark, that the oxide at a maximum, is frequently obtained perfectly white. For example, if the filings of tin, and the red oxide of mercury, are mixed together, and exposed to fire, or if the purple powder of cassius be ignited, and the gold afterwards dissolved by the nitro-muriatic acid. This white oxide does not change its colour when ignited in the fire, and it is insoluble in muriatic acid, which receives from it a yellowish tinge. I imagined at first, that this oxide was in a still higher degree of oxidation, but I have not been able to discover whether the tin gains more weight when oxided by the oxide of mercury, which gives this white oxide, than when it is oxided by nitric acid. The only difference between the yellow and the white oxide, when both are at a red heat in the fire, appears then to consist in a different aggregation, exactly as *Sefström* has proved, that the black precipitate, which is obtained from corrosive sublimate, by means of sulphureted hydrogen gas, contains the same constituent parts in exactly the same proportion as cinnabar, and, consequently, the colour is to be attributed to the difference of aggregation.

Combinations of the Oxides of Tin with the saline bases.

I have already mentioned, that the hydrate of the yellow oxide colours the paper of tournesol red, but that the oxide that has been through the fire, loses this quality. I have reason to suppose, that it is the same with intermediate oxide. If we add to this, the property possessed by these oxides of producing with the alkalies, combinations which are soluble in water, we shall have plausible reasons to consider all these oxides as acids, exactly as we have already done, with the oxides of antimony. But, as on the other hand, these oxides produce, with the acids, salts which are quite neutral, and are the weakest amongst the electro-negative bodies, it is difficult to decide what name should be given them in preference. In the manner in which these oxides comport themselves with the alkalies, and saline bases, they so much resemble each other, that I have not been able, in my experiments, to discover any difference, except that the combinations of the intermediate oxide with

with alkalis, earths, and metallic oxides, are soluble in acids, whilst the combinations of the oxide at a maximum, always leave the hydrate of the oxide undissolved. Oxides of tin.

The hydrate of the oxide at a maximum, is easily dissolved in caustic alkali. The solution produces crystals by evaporation, which are white, and in the form of grains, and have a caustic and alkaline taste, but as it was not possible to purify them from the very concentrated lixivium in which they were deposited, I have not been able to analyse them. If, on the contrary, the alkali be diluted with water, and as much oxide of tin added to it as the alkali will dissolve, a fluid is obtained which is of a dusky yellow colour, and when looked at through the light, becomes opal and whitish. This liquor does not afford crystals, but on evaporation forms a jelly which dries slowly, and then becomes a yellowish mass, which is soluble again in water. At a red heat this combination is decomposed, the oxide becomes insoluble, and the alkali remains and may be separated from it by water. If, on the contrary, this mass is burned with an excess of alkali, it becomes white, and the oxide is rendered soluble in acids. This experiment proves, that the affinity of the oxide of tin is so weak, that heat deprives it of the power of remaining in combination, and reduces it to a state of insolubility, notwithstanding the affinity of the bodies with which it is actually combined. The effect of an excess of alkali in the fire seems, in part, to be analogous to what we see in silex, alumine, &c. and in part depends on the circumstance, that the oxide is reduced to an intermediate degree of oxidation.

(To be continued.)

VII.

METEOROLOGICAL JOURNAL.

1813.	Wind	Barometer. Max.	Min.	Med.	Thermometer. Max.	Min.	Med.	Evap.	Rain	
3d Mo.										
Mar. 25	N W	30·37	30·28	30·325	47	35	41·0	—		☾
26	N W	30·50	30·37	30·435	51	27	39·0	—		
27	S W	30·47	30·43	30·450	55	32	43·5	—		
28	N W	30·43	30·30	30·365	67	49	58·0	—		
29	S W	30·30	30·10	30·200	66	53	69·5	—		
30	Var.	30·10	29·89	29·995	58	47	52·5	—	1	
31	S E	29·89	29·18	29·535	57	42	49·5	40		
4th Mo.										
April 1	S W	29·25	29·18	29·215	50	35	42·5	—	·27	●
2	W	29·45	29·25	29·350	52	35	43·5	—	9	
3	S W	29·74	29·45	29·595	48	27	37·5	—	3	
4	S W	29·85	29·74	29·795	54	29	41·5	—		
5	S W	29·85	29·81	29·830	51	40	45·5	—	·13	
6	S W	29·90	29·74	29·820	58	45	51·5	—		
7	W	29·93	29·90	29·915	65	43	54·0	·43	—	☽
8	E	29·97	29·87	29·920	69	37	53·0	—		
9	S E	30·04	29·97	30·005	66	41	53·5	—		
10	E	30·10	30·04	30·070	65	41	53·0	·26		
11	E	30·14	30·10	30·120	64	35	49·5	—		
12	E	30·23	30·14	30·185	69	42	55·5	—		
13	N E	30·34	30·23	30·285	66	35	50·5	—		
14	E	30·20	30·10	30·150	66	42	54·0	·36		
15	N W	30·20	29·96	30·080	68	42	55·0	—		○
16	N W	29·96	29·77	29·805	66	44	55·0	—		
17	N W	30·10	29·77	29·935	68	41	54·5	—	—	
18	N W	30·13	30·10	30·115	56	42	49·0	·53		
19	N W	30·10	30·10	30·100	64	44	54·0	—		
20	N W	30·10	30·05	30·075	64	40	52·0	—		
21	N	30·09	30·05	30·070	57	32	45·5	—		
22	N E	30·10	30·08	30·090	50	32	41·0	—	·17	
23	N E	30·14	30·10	30·120	45	34	39·5	·40		
		30·50	29·18	30·005	69	27	49·11	2·38	0·70	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Third Month. 27. Hoar frost: large spreading *Cirri.* 28. Temperature 60° in the evening. 29. Overcast sky. 30. A veil of *Cirrostratus* a. m. The *Cumulus* afterwards shewed itself, and a slight shower ensued.

Fourth Month. 1. Stormy, with rain. 2. Hoar frost: a sprinkling of opaque hail about sun-rise. Several showers of this and some rain during the day. 3. Hoar frost: *Cumulus* a. m. Showers of snow, and of opaque hail p. m. 5. a. m. *Cirrostratus*, wet and windy. 10. *Cirrus* and *Cumulus* clouds: the N. E. wind increases in strength: the mornings have been misty of late, and there have been plentiful dews, in consequence of the great difference between the temperature of day and night. 15. Wind boisterous in the evening 16. Cloudy a. m. 17. Slight showers. 20. From the 7th of this month we have had summer-like days and cold nights: the roads have become very dusty, and the earth considerably dry. 21. Some clouds of a threatening appearance from the N. E. in the evening attended with depression of temperature. 22. p. m. hasty showers, mixed with hail; after which steady small rain till evening. 23. Cloudy: several scanty hail showers from large Nimbus clouds passing over. During the approach of one of these, a slender tapering, and somewhat twisted column, appeared in front, detached from the main body, and reaching down to the earth in the manner of a water spout. In a few minutes, by spreading on all sides, it became incorporated with the rest of the shower. This is not a very uncommon appearance, but I have seldom seen it so perfectly exhibited.

RESULTS.

Prevailing winds Westerly, interrupted (after the middle of the period) by an Easterly current.

Barometer: greatest observed height, 30·50 in.; least 29·18 in.

Mean of the period 30·005 inches.

Thermometer: greatest height 69°; least 27;

Mean of the period, 49·11°.

Evaporation 2·38 in. Rain 0·70 in.

L. HOWARD.

TOTTENHAM,
Fourth Month, 25, 1813.

VIII.

On the Conducting Power of Bodies for Electricity, and the Effect of Points. By R. B.

To Mr. Nicholson.

SIR,

THE importance of electrical knowledge in chemistry, makes it desirable that philosophers should direct their attention to the general effects of this once very fashionable, and still surprizing department of experimental knowledge. This will, I am confident, obtain your admission for the following, and perhaps other, communications, I may hereafter send you.

Electric signs. It remains still undecided, whether the electric signs be caused by one single fluid, or by two fluids; whether the luminous appearances be produced by the electric matter rendered visible, or by a deflagration or combustion of the conductors, or of one of the elements of the air during its course, or by change of its density by mere impulse; whether bodies have any attraction for this luminous matter, which flies to them, or whether they retain it merely by virtue of the surrounding air, and readily give it out in a vacuum; whether there be any repulsion between the particles of electricity, since the luminous stream in vacuo does not diverge, and its distribution in conductors as in the electric well, follows another law than would arise from repulsion? and while these and other very important doubts remain; what theory can be adopted, or what peculiar nomenclature can be made use of, which will not, by favouring some undecided point, lead us into error?

Excitation; Elect. charges, simple electricity. The order in which electrical phenomena are produced, is not the most eligible to consider them in. Excitation, which is the first operation the electrician performs, is the most abstruse and surprizing of all the appearances; and it requires a most intimate acquaintance with the electrification of simple conductors, and the nature of electric charges, before any conjecture can

can be made with the least probability of success respecting it. It seems proper that our enquiries and observations should be first directed to the simplest electric state of conductors; in the next place to electric charges, and lastly to excitation.

Conductors. Conductors of electricity may be considered in three points of view; 1. They afford a passage to the electric matter; 2. They admit of a variation in the quantity of electricity they contain, and acquire by that means what is called a state of electricity; 3. And they admit of a variation in their capacity for retaining electricity, and by that means acquire an electric state, though the quantity of electricity they contain does not probably suffer any increase or diminition. This is, what is usually called, the electric charge, and perhaps depends entirely on the non-conductor, which, in all experiments, must surround or envelope the conductors made use of.

Their nature. Electricity passes from place to place, either invisibly through or over conductors, or visibly along the surfaces of bodies. If a conductor be very small, in proportion to the quantity of electricity, it transmits the electricity visibly, whether its own substance be dissipated or not. Astronomical silver wire, of about the eight hundredth part of an inch thick, is very convenient for making experiments of this kind with small jars. Those bodies which are called non-conductors, do all conduct electricity, both invisibly and visibly; and it is known that this power resides at their surface, because glass balls charged and sealed up, retain their electricity for many years, and the sealing does nothing more than destroy the connection between the outer and inner surfaces. Hence it appears probable, that electricity is in almost every case, transmitted along the surfaces of bodies, and is not capable of flying over considerable intervals without such a surface to conduct it. But the sensation experienced in the electric shock, seems to shew that it can pass through the internal parts of conductors.

They probably act by surface only. From this conclusion we may conceive, how a non-conductor may become a better conductor at a certain density than at any other. Thus for example, dense air resists the passage of electricity, because its particles are too close together to admit of free surfaces, without their being removed from each other, as is effected by the electric spark. If its rarity be greater the surfaces are more remote, and probably exert their conducting power

power better: and if the rarity be still greater, the leaf from one particle to another may be too great for the electricity to pass by the intensities we produce. The impermeability of a vacuum to electricity, seems strongly to countenance this doctrine.

Doubt whether conductors attract electricity.

The next point of view in which conductors are to be considered, relates to the electric state they are capable of. This is usually supposed to be a natural consequence of an attraction, exerted between the conductor and the electric matter; but a more accurate view of the appearances will shew that this principle is by no means the leading agent in producing, or maintaining electric intensity. For the electric sparks a foot or more in length, scarcely ever strike the nearest part of the receiving ball, or pass by the nearest course; pointed conductors emit or receive electricity, with the greatest facility; and no conductor retains its electric state in a common boylean vacuum. Since, therefore, it appears that conducting bodies exert a very inconsiderable force of attraction on the electric matter, it remains to be enquired, by what powers they retain the intensity they may be made to acquire.

The air maintains electric intensity.

From the facility with which the electric matter traverses a vacuum, or rarified air, it is evident that the preservation of electric intensity in bodies depends greatly on the resistance afforded by dense air to its escape; but at the same time the phænomena of pointed bodies, shew that the co-operation of another circumstance is essentially necessary to the effect. What this circumstance is must be deduced from the solution of one of the most important, and most difficult, questions in electricity, that is to say, why do pointed bodies throw off, or receive electricity with the facility they are known to possess in this respect? For the other question, why do blunt bodies retain electricity? is the converse of this.

Effect of pointed bodies.

The effects of pointed bodies have been accounted for in several ways, but the following are the most generally received. They who admit of electric atmospheres, shew that the atmosphere surrounding an angular termination, admitting its altitude to be every where the same on a given conductor, is really larger than would be required to envelope the less prominent parts of the surface, and thence they infer, that the escape or admission of electricity must be the easiest of all, at such terminations. This

This explanation labours under the difficulty of its being hitherto far from being proved that such atmospheres have any existence; and likewise under that of the supposition of an uniform height of such atmospheres, and their disposition to fly off, without any other evident cause than an accumulation, which apparently ought to be preserved by the same cause which occasioned it, being entirely gratuitous. Volta ascribes the action of points to the minuteness of their surface, which does not admit any sensible quantity of the electricity to take the form of a charge,* as it always does in large surfaces, to the diminution of that intensity on which the disposition to escape depends. This explanation is nearly the same as if he had said, that the surfaces between which electricity passes, are the coatings of an electric plate of air, and that the transition consists in the breaking of this plate of air, as soon as it is highly enough charged; that the smaller the surface, the higher the charge, the same power of excitation or accumulation of electricity, being supposed, and, consequently, the thicker the plate of air which might be broken through, or distance to which the electricity would fly. That the escape or transition of electricity is prevented or rendered less easy by the diminution of intensity, produced by the vicinity of an uninsulated conductor is experimentally proved; but it does not appear probable, that the want of this diminution is the principal cause of the action of points. For if we suppose a round ball to be presented to an electrified conductor, at such a distance as that the diminished intensity should be insufficient to produce a communication of electricity, it ought to follow, if the diminution were the principal cause of the communication not taking place, that a point at the same distance would cease to receive or emit electricity, as soon as its operation had produced as great a diminution of intensity as the ball had produced in the former case; or that a ball, presented at a given distance from a conductor, would prevent the operation of a point. But it is well known, that the facts by no means confirm this deduction, not to mention that a ball of a moderate size produces no sensible diminution of intensity in a large conductor well supplied; and that, in many cases, pointed bodies perform their usual effect at great distances from other

* Philosophical Transactions, vol. lxxii. p. 1.

bodies

bodies, which are supposed to possess the intense opposite state, and do not seem to act much, if at all, less effectually on that account. I must likewise add, that if a point be made to issue from the surface of a metallic ball, and that part of the ball which is diametrically opposite the point, be presented to an electrified conductor, the point, though not directed towards the conductor, will, nevertheless, discharge its electricity silently, in the usual manner, though no good reason can be offered to shew why the ball, which by its charge is supposed to prevent the transition of electricity to or from its own surface, should not equally affect the remoter point.

Lord Stanhope's theory of atmospheres of electrified air.

Earl Stanhope, admitting the existence of electrical atmospheres, consisting of air electrified by communication, accounts for the action of points from the circumstance that their extremities, projecting beyond the common surface, are placed in a less dense part of the atmosphere, where the resistance to the transition of electricity is less than nearer the surface. On this hypothesis I must observe, that it is not probable that electrified bodies have atmospheres of air electrified by communication, for two very strong reasons. The first is, that if such communication took place almost instantly, to the distance of many feet, it would be scarcely possible to shew why the whole electricity of a conductor should not be conveyed off in the smallest space of time; and it is admitted that electric atmospheres extend as far as their effects are perceived. In the second place, it is known that the effects of electricity diminish gradually and regularly as the distance increases, and that air receives and loses electricity slowly;—two circumstances which are not reconcileable with the little disturbance which agitation of the air produces in those effects. For if the slowly electrified air were the cause of the general phenomena of electricity, it would doubtless cease to act with regularity, when the more or less electrified parts were mixed together. But though the position of the existence of such atmospheres so formed, seems inadmissible, yet it will appear, upon consideration, that the sagacity of the noble author has led him to point out the fact, on which the action of points principally depends.

A point considerably elevated above the surface of an electrified conductor, emits or receives electricity with scarcely any sound, and with great facility. When very little elevated, this

this facility is much less, and the transition produces more noise; and, when an elevation is about the $\frac{1}{30}$ of an inch, with positive electricity, the point throws out sudden explosive brushes, and if an uninsulated ball be presented, it emits long zig-zag sparks. The blunter the point, the less it need be depressed to produce this change in its property, and some variations may perhaps follow from variations of intensity. With negative electricity the point does not lose its property of causing a silent transmission of electricity, till it is very much more depressed than in the experiments with positive electricity. As a point is in effect a surface so small that one machine can cause the electric matter to pass off in a continual flux, it is evident that with a great intensity a ball may even be considered as a point.

These are the leading facts respecting points, and they do not appear capable of being explained by any simple statement. To me they seem to depend on a fact which is the reverse of the electric charge; but I must reserve this to a future letter, and am,

SIR,

Your obliged Reader,

R. B.

SCIENTIFIC NEWS.

Geological Society.

May 7th, 1819.

The President in the Chair.

Matthew Cully, Esq. of Askeld, Northumberland,

Thos. Brandram, Esq. of Lee, Kent,

Were severally elected members of the society.

The reading of Dr. Mac Culloch's paper on the Geology of certain parts of Scotland, was begun.

The

The first article in this paper treats of the granular quartz rock of the island of Jura. This, by some denominated granite, and by others, granular quartz, but by all who have hitherto described it, considered as a primitive rock, constitutes the principal and fundamental rock of the island; in particular, the three well known conical *paps of Jura*, of the height of 2500, or 2600 feet, are entirely composed of this mineral.

It is disposed in regular uninterrnpted strata, six or eight feet in thickness, and rising, for the most part, at a considerable angle towards the west. These strata do not appear to be traversed by veins, except of quartz, nor do they alternate with any other rock. On the shore, however, the dip and direction of the beds are observed to vary considerably. The mineralogical composition of this rock, presents several varieties. Sometimes it is extremely compact, being made up of grains of quartz, of various degrees of magnitude, united without cement. Sometimes, besides the quartz, it contains felspar, seemingly in rounded fragments, and often decomposed into clay. In one specimen, a manifestly water worn pebble of quartz is enclosed; and, upon the whole, the rock may be considered as a kind of sand-stone, consisting of quartz and felspar, the former in the larger proportion. In some of the beds, the sand-stone passes into granwacke-slate, by mixture with pieces of mica-slate. From these circumstances, D. M. considers the quartz rock of Jura as a mechanical deposit formed from the fragments of older ones, and not as belonging to the Wernerian primitive class. According to Professor Jameson, however, this very rock rises from below the micaceous schistus; we must therefore admit either that the micaceous schistus described by Professor J., is not primitive, or that the circumstances under which the primitive rocks were formed, were such as to exclude, at the same time, the production of a mixed mechanical deposit.

The next article in this paper, contains some miscellaneous remarks on the geology of the island of Rona. The principal rocks that here make their appearance, are gneiss and hornblende rock (including, under the latter denomination, both hornblende-slate and greenstone-slate). Where these two rocks come in contact, the gneiss is irregularly curved and contorted; the gneiss

gneiss is traversed by numerous and thick veins of graphic granite, in which wolfram occurs.

The district of Assynt, forming the western part of Sutherlandshire, is the subject of the next article. The mountains and higher ground of this district, consists of the same rock as the so called granular quartz of Jura, forming here, as in the last mentioned island, smooth conical hills of considerable elevation, snow white at their summits, and singularly sterile and arid. The white colour of the rock, is, however, only superficial, the recent fracture exhibiting grey, yellow, and brown tints. It is distinctly stratified, and rises at a high angle. The texture of this rock is various, from imperfectly conchoidal, to loosely granular, composed of rounded grains, and in some beds of angular fragments. It divides naturally into rectangular blocks, on the surface of which, is the appearance, as if of cylindrical bodies imbedded in the moss, forming a number of circular protuberant spots, of a whiter colour, and more compact texture, than the rest of the rock. A section at right angles, to the natural surface of these blocks, shews that the above-mentioned circular spots are occasioned by the cross fracture of strait cylindrical bodies, which are, perhaps, the remains of some species of sabella. Associated with this grit, are compact gneiss, hornblende-slate, and syenitic granite, but their relative positions, D. M. was unable to ascertain. Subordinate to, and apparently alternating with this grit, is a great deposit of limestone, in two very thick stratified beds, with a thick bed of grit interposed. In some parts, the section of these beds forms a continuous and even line; but in other parts, is so curved and broken, that the stratification can scarcely be perceived.

The limestone is dark, grey, or nearly black, of an earthy aspect, and minute granular fracture, and smelling offensively when rubbed. It does not appear to contain organic remains, but is traversed by veins of red or white calcareous spar; it contains grains of sand, and, therefore, gives fire with steel. Its surface is covered, for the most part, with a loose calcareous tufa, which, in some places, being rendered solid by an infiltration of calcareous matter, constitutes a hard breccia.

In the same valley of the Tain, of which the above rock forms the precipitous side, occur insulated masses, rising through the

the grass of instratified granular marble, varying in colour from pure white to gray, the geological relation of which, Dr. M. has not been able to determine. This is the white marble mentioned by Williams in his "Mineral Kingdom," and which has since been wrought with some success, by Mr. Sophir of Gateshead.

May 21, 1813.

The President in the Chair,
William Hill, Esq. Bedford Row,
Hastings Elwin, Esq. of Farnham, Dorset,
Frederic Daniell, Esq. of Lincoln's Inn Fields, were severally elected members of the Society.

A paper by the Rev. William Greg, Hon. M. G. S. containing "Observations on a species of Tremolite found in Cornwall," was read.

This mineral occurs in a dark green serpentine rock forming the ridge called Clicker tor, in the neighbourhood of Liskeard. It is accompanied by Asbest. On analysis it appears to be composed of

62·2	silicia
14·1	lime
12·9	magnesia
5·9	oxid of iron
1·0	water
A trace of oxide, of manganese, and of soda.	
96·1	
3·2	loss
100 0	

The continuation of Dr. Mac Culloch's paper on the geology of differents parts of Scotland was read, and thanks were voted for the same.

The granular quartz of Isla appears to be precisely the same rock, or the sandstone of Jura, already mentioned. From the observations of Pr. Jameson, coinciding with those of Dr. Mac Culloch it appears to alternate with mica slate and clay slate, and with a very important formation of limestone. This limestone

stone is more or less granular, and contains no organic remains, nor any beds of fetid limestone: when enclosed between beds of clay slate, it is of a dark blue colour: when in contact with mica slate it is grey or white; both varieties pass insensibly into the slate within which they are enclosed: and the limestone, the schistus, and the sandstone, are evidently members of one formation.

The structure of Schehallien is the subject of the next article. This mountain consists of a central ridge in vertical strata flanked on every side by beds of mica slate nearly vertical, and containing subordinate beds of limestone. The rock composing this central ridge, though it has been denominated granite by some mineralogists of no mean name, is in fact the same as the granular quartz of Jura, being composed of highly compacted grains of quartz with interspersed grains of earthy felspar. The same quartz rock appears in the valley of the Lyon to the S. of Schehallien, and it seems that the mica slate alternates with beds of quartz rock; and is, therefore, of the same æra as this latter.

The vicinity of Criman, which is the subject of the next article, is remarkable for presenting nearly vertical beds of well characterized granwake and granwakke slate, with equally well characterized beds of clay slate and chlorite slate.

The structure of the rocks bounding the vale of Abesfayle, is next described. On tracing this country up to Benledi alternations of granwakke and granwakke slate with clay slate first occur: then comes a fine roofing slate approaching in parts to mica slate, but distinguished by a true granwakke structure, that is of grains united by a slaty cement, only in this case the cement is not clay slate but mica slate: beyond this, the true mica slate makes its appearance.

The general deduction from these facts is, that those rocks which have been ranked as primitive schist alternate with rocks of recomposed materials, which belong to the transition class of Werner: but this alternation throws great doubt on the reality of transition rocks as distinguished from primitive, and rather tends to bring back the original division of rocks into primitive and secondary.

ERRATA.

ERRATA

To Volume XXXIV.

page	line	
280	3	*for* " olive oil by boiling," *read* olive oil thickened by boiling.
280	29	*for* " or water," *read* on water.
282	3	*for* " Do. precipitation of arsenic," *read* Do.
289	29	*for* " ·773 azote," *read* ·073 azote.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

JULY, 1813.

ARTICLE I.

A Theory of the Tides, including the Consideration of Resistance. By a Correspondent, E. F. G. H.

[The author of these investigations is sensible that they are not altogether so explicitly and demonstratively detailed as could be desired; they were not written with any immediate view to publication; but, as they contain some new results, and may possibly lead to new methods of calculation, he thinks it better that they should be printed in an imperfect form, than that they should be wholly lost, which is the only alternative compatible with his present engagements. He does not apologize to an author from whom he has borrowed some ideas, because all those, who are sufficiently interested in the subject to study this essay, are probably already acquainted with that author's works.]

THEOREM A. Fig. 1. Plate IV.

Forced vibrations of a pendulum.

IF the point of suspension (A) of a pendulum (AB) be made to vibrate in a regular manner, that is, according to the law of cycloidal vibrations, the pendulum itself may also vibrate regularly in the same time, provided that the extent of its vibrations (BC) be to that of the vibrations of the point of sus-

suspension (AD), as the length of the thread (AE), supposed to carry this point, is to the difference of the lengths of the two threads.

In representing the vibrations, we may disregard the curvature of the paths, considering them as of evanescent extent, the forces being still supposed to depend on the inclination of the threads. Let F be the intersection of AB with the vertical line EF, then, upon the conditions of the theorem, BF will be equal to AE, since BC : AD=AE : AE - AB, and, by the properties of similar triangles BC : AD=BF : BF - AB. Consequently the inclination of the thread AB will always be the same as if F were its fixed point of suspension; and the body B will begin and continue its vibrations like a simple pendulum attached to that point, the true point of suspension accompanying it with a proportional velocity, so as to be always in the right line passing through it and through F. It is obvious, that when the thread suspending the moveable point of suspension is the longer of the two, the vibrations will be in the same direction; when the shorter, in the contrary direction.

Illustration. *Scholium* 1. The truth of this proposition may easily be illustrated, by holding any pendulous body in the hand, and causing it to vibrate more or less rapidly, by moving the hand regularly backwards and forwards.

Application to other forces. *Scholium* 2. The same mode of reasoning is applicable to oscillations of any other kinds, which are governed by forces proportional to the distances of the bodies concerned, from a point of which the situation, either in a quiescent space, or with respect to another moveable point, varies according to the law of the cycloidal pendulum, or may be expressed by the sines of arcs varying with the time: such forces always producing periodical variations, of which the extent is to that of the excursions of the supposed point of suspension in the ratio of n to $n-1$, n being to 1 as the square of the time of the forced, to that of the time of the spontaneous vibration, and when $n-1$ is negative, the displacement being in a direction opposite to that of the supposed point of suspension. Consequently, when a body is performing oscillations by the operation of any force, and is subjected to the action of any other periodical forces, we have only to inquire at what distance a moveable

able point must be situated before or behind it, in order to represent the actual magnitude of the periodical force by the relative situation according to the law of the primary force concerned, and to find an expression for this distance in terms of the sines of arcs increasing equably, in order to obtain the situation and velocity of the body at any time, provided that we suppose it to have attained a permanent state of vibration.

Scholium 3. If the oscillating body be initially in any other condition, its subsequent motion may be determined, by considering it as performing a secondary vibration with respect to a point vibrating in the manner here supposed, which will consequently represent its mean place; but if there be no resistance, the body will have no tendency to assume the form of a regular simple vibration, rather than any other. Different case.

THEOREM B.

If the resistance be simply proportional to the velocity, a pendulum with a vibrating point of suspension may perform regular vibrations, isochronous with those of the point of suspension, provided that, at the middle of a vibration, the point of suspension (A) be so situated as to cause a propelling force equal to the actual resistance, the extent of the vibrations being reduced in the ratio of the whole excursion of the point of suspension (BC) to its distance from the middle at the beginning of the motion of the pendulous body (DC): and it will ultimately acquire this mode of vibration, whatever may have been its initial condition. Forced vibrations; with resistance as the velocity.

Let FG, fig. 3 and 4, be the supposed length of the thread carrying the point of suspension, and draw FE passing through D; then if HC=EG be the extent of the vibration, it will be maintained according to the law of the cycloidal pendulum. Draw the concentric circles BI, DK, HL: now the initial force may be represented by HD, which determines the inclination of the thread; and at any subsequent part of the vibration, if the centre be advanced from D to M, the time elapsed will be expressed by the arc IN; DI and MN being perpendicular to AB; and taking HL similar to IN, the perpendicular LP will show the place of the pendulous body, and PM the force, which may be divided or resolved into two parts, PQ and QM. But PQ is to LK, or HD, as PC to LO, or HC; consequently

 this

this part of the force will always be employed in generating the regular velocity; and QM is equal to KR, which is the sine of the angle KNR or BCL to the radius KN=DI=AC, and which therefore varies as the velocity, and will always be equal to the friction, provided that it be once equal to it, the ratio of the forces concerned in any two succeeding instants being always such as to maintain a regular vibration.

If the pendulum be initially in any other situation than that which is here supposed, its subsequent motion may be determined by comparison with a point vibrating regularly: and if we wish for a general view of the case in an early stage, the effect of resistance in these secondary vibrations with respect to such a point, may be neglected: but since they are not supported by any sustaining force, they will evidently be rendered by degrees smaller and smaller, so that the pendulum will ultimately approach infinitely near to the regular state of vibration here described, which may therefore be considered as affording a stable equilibrium of motion.

Magnitude of displacement.

Scholium 1. Supposing the relation of the resistance to the velocity to be altered, the relation of the sine AC to the cosine CD must be similarly altered, the force equivalent to the resistance varying as the sine, and the extent of the vibrations, and consequently the velocity, as the cosine of the displacement BI: but the relation of the sine to the cosine is that of the tangent to the radius: so that the tangent of the displacement will be as the mean resistance. And the sine of the displacement AC is to the radius BC as the greatest resistance is to the greatest force which would operate on the pendulous body if it remained at rest at G.

Different demonstration.

Scholium 2. This proposition may also be deduced from the former, by representing the resistance as a force tending to a moveable centre of attraction, analogous to the point of suspension of a pendulum, so as to create a new vibration liable to an equal resistance; or still more simply in the present instance, by attributing the whole actual resistance to the principal vibration, and considering the subordinate vibration as exempt from it. The resistance at G may evidently be represented by the force acting on a pendulum of the length AG at the distance AC from the vertical line, and the corresponding excursion of the pendulous body must be represented, according

cording to the former proposition, by GS, which is to AC as the length of the thread corresponding to the periodical time is to the difference of the lengths: so that when the place of the body, as determined by the former proposition, without resistance, would have been S, it is actually found in G: the centre of attraction representing the resistance being always behind the body, the body will also be behind the place which it would have occupied without the resistance when the vibration is direct, but before it when inverted: and it will be found, that the forces concerned preserve their due proportion in every other part of the vibration. At the beginning of the true vibration, the body must have its greatest velocity in the subordinate vibration representing the effect of resistance, and this velocity must be equal and contrary to the supposed velocity in the primitive vibration, independent of resistance; consequently AC, representing the greatest velocity in the subordinate vibration, must be equal to DI, the sine of the displacement, which shews the velocity in the primitive vibration. And this agreement with the former demonstration is sufficient to show the accuracy of this mode of representing the operation of the forces concerned.

THEOREM C.

Resistance as the square of the velocity.

If the resistance be proportional to the square of the velocity, a pendulum with a vibrating point of suspension may perform vibrations isochronous with those of the point of suspension, and very nearly regular, the relative situations being nearly the same as in the case of a similar pendulum liable to a resistance simply proportional to the velocity, and equal in its aggregate amount to the actual resistance.

The mode of investigation which has been exemplified in the last scholium, may be applied to this and to all other similar cases; the only difficulty being of a mathematical nature, since the method depends on the expression of the forces concerned in the terms of sines or cosines of arcs, and their multiples; and it appears to be frequently impossible to do this otherwise than by approximation. In the present instance we cannot obtain a perfectly correct expression for the square of the sine: the square of the sine, in the common language of mathe-

mathematics, being always positive, and this case requiring an alternation of positive and negative values, the common forms employed by Euler, Arbogast, and others, completely fail; and the difficulty seems to be rather in the nature of the problem, than in the mode of investigation, for the formula which answers the conditions most completely for one part of the circle, seems to be incorrect at others. Thus we may put $S^2x=aSx+bS3x+cS5x\ldots$ omitting the even multiples, since they would afford different values for the corresponding parts of the first two quadrants, and take the successive orders of fluxions, whence we have

$S^2x=aSx+bS3x+cS5x+\ldots$

$2Sx\varsigma x= a\varsigma x+3b\varsigma 3x+5c\varsigma 5x+\ldots$

$2-4S^2x=-a\varsigma Sx-9bS3x+25cS5x\ldots$

$-8Sx\varsigma x=-a\varsigma x-27b\varsigma 3x-125c\varsigma 5x\ldots$

$16S^2x-8=aSx+81bS3x+625cS5x\ldots$

If in these five equations we make x alternately $=90°$, and $=0$, we may find five coefficients, $a=\cdot 7861$ $b=-\cdot 2598$, $c=-\cdot 03709$, $d=\cdot 01612$, and $e=\cdot 00732$, which represent the ordinates of a curve agreeing with the curve proposed at the vertex and at the origin in situation, in curvature, and in the first and second fluxions of the curvature: and yet the curves differ surprisingly from each other in the intermediate parts; the ordinate at 45° becoming less than ·4 instead of ·5.

Approximation.

On the whole, the best mode of determining the coefficients viz.(·4)(·5) appears to be, to divide the quadrant into as many parts as we wish to have coefficients, and to substitute the corresponding values of the arc in the general equation; we thus obtain $a=\cdot 8484$, $b=-\cdot 1696$, $c=-\cdot 0244$, $d=-\cdot 0081$, $e=-\cdot 0029$, and $f=-\cdot 0013$. Then if we make, as before, the square of the time in the entire forced vibration of the point of suspension to the square of the time of the spontaneous vibration of the pendulum as n to 1, the distance of the pendulous body will be expressed by $\frac{n}{n-1}$ when that of the point of suspension is unity; and accordingly as $n-1$ is positive or negative, the body will be on the same side of the vertical line with the point of suspension, or on the opposite side: and the same will be true with respect to the displacement corresponding to the first term of the series expressing the resistance, substituting

the

the supposed centre of attraction for the point of suspension, and the mean place for the vertical line: but in the following terms, the value of n is successively reduced to $\frac{1}{9}$, $\frac{1}{25}$, and so forth: consequently, the whole displacement immeditely produced by the effect of the resistance rS^2x will be $rn \left(\frac{\cdot 8484}{n-1} Sx - \frac{\cdot 1696}{n-9} S3x - \frac{\cdot 0244}{n-25} S5x - \frac{\cdot 0081}{n-49} S7x - \frac{\cdot 0029}{n-81} S9x - \frac{\cdot 0013}{n-121} S\,11x\right)$. This displacement will, however, cause an alteration of the resistance, which may be considered as a differential of the former, and since $(y^2)^{\cdot} = 2y\dot{y}$, the new resistance may be expressed by the product of the new and twice the original velocity, or by $-2r^2nsx\left(\frac{\cdot 8484}{n-1} \varsigma x - 3\frac{\cdot 1696}{n-9} \varsigma 3x - 5\frac{\cdot 0244}{n-25} \varsigma 5x - \ldots\right)$, and the consequent displacement may be determined in the same manner as for the original resistance. The first term gives $2r^2n^2 \frac{\cdot 4242}{(n-1)\cdot(n-4)} S2x$: for the remainder we must find an equivalent series in terms of the cosines of multiple arcs, since the direction of the force of resistance does not change where the sine becomes negative: and each term will require a separate investigation while n remains indeterminate; but for the present purpose two of the terms will be sufficient. The method employed by Euler for determining the coefficients in such cases is of no use here, because it affords a progression of sines only: we must, therefore, put $a\varsigma x + b\varsigma 3x + c\varsigma 5x + d\varsigma 7x$ successively equal to $Sx\varsigma 3x$, and to $Sx\varsigma 5x$; then, bisecting and trisecting the quadrant, we find the coefficients ·1667, — ·3333, ·6869, and — ·5202, and — ·1057, — ·2886, ·6421, and — ·2478, respectively, and the whole of this second displacement becomes $2r^2n^2\left(\frac{\cdot 4242}{(n-1)\cdot(n-4)} S2x - \frac{\cdot 5088}{n-9}\left(\frac{\cdot 1667}{n-1}\varsigma x - \frac{\cdot 3333}{n-9}\varsigma 3x + \frac{\cdot 6869}{n-25}\varsigma 5x - \frac{\cdot 5202}{n-49}\varsigma 7x\right) - \frac{\cdot 1220}{n-25}\left(-\frac{\cdot 1057}{n-1}\varsigma x - \frac{\cdot 2886}{n-9}\varsigma 3x + \frac{\cdot 6421}{n-25}\varsigma 5x - \frac{\cdot 2478}{n-49}\varsigma 7x\right)\right)$.

For example, if we take r and n, each equal to $\frac{1}{2}$, the first Example.
formula gives —·4434 for the displacement at the middle of the time, and 0 for the beginning; the second 0 at the middle, and —·0022 at the beginning: but the true beginning of the actual vibration is modified by the velocity belonging to the first order of the effects of resistance, which is found in this case ·390, consequently, the true time of rest will be when the velocity is —·390 in the primitive vibration, or when the arc corresponding to the excursion is 67°, and its sine ·920, which, lessened by ·0022,

·0022, shews the true extent of the excursion ·9178; and reckoning from this point as the beginning, the displacement in the middle will be reduced to about ·05. Now an equal mean resistance, varying simply in proportion to the velocity, would cause a displacement in the middle of ·3957 instead of ·4434; and reckoning from the true beginning of the vibration, the displacement in the middle would vanish, instead of being reduced to about ·05, and the extent would be ·9196 instead of ·9178. And if r were smaller than $\frac{1}{2}$, there would obviously be still less difference in the two cases. From the small proportion which the second displacement bears in this case to the first, it may be inferred, that any further calculation, of the effects of the third order, would be wholly superfluous.

Failure of direct investigation.

Scholium 1. Dr. T. has suggested an ingenious method, which affords a formula for the coefficients of the first series; but unfortunately it loses its convergence too soon to be of any use. Taking the equation $2Sx\varsigma x = a\varsigma x + 3b\varsigma 3x + 5c\varsigma 5x + \ldots = 2(1-\varsigma^2 x)^{-\frac{1}{2}}\varsigma x$, we may expand this expression, by means of the binomial theorem, into $2\varsigma x - \varsigma^3 x - \frac{1}{4}\varsigma^5 x - \frac{1}{8}\varsigma^7 x - \ldots$ and substituting the cosines of multiple arcs for the powers of ςx, and then comparing the homologous terms, we obtain $a = 2(1-\alpha-\beta-\gamma-\delta-\ldots)$ where $\alpha = \frac{3}{16}$, $\beta = \frac{1}{4}.\frac{1}{2}.\frac{5}{3}\alpha$, $\gamma = \frac{1}{4}.\frac{3}{3}.\frac{7}{4}\beta$, $\delta = \frac{1}{4}.\frac{5}{4}.\frac{9}{5}\gamma$, $\varepsilon = \frac{1}{4}\frac{7}{5}.\frac{11}{6}\delta \ldots$: $b = -\frac{2}{3}(\alpha+\beta+\gamma+\ldots)$ where $\alpha = \frac{1}{16}$, $\beta = \frac{1}{4}.\frac{1}{1}.\frac{5}{4}\alpha$, $\gamma = \frac{1}{4}.\frac{3}{2}.\frac{7}{5}\beta$, $\delta = \frac{1}{4}.\frac{5}{3}.\frac{9}{6}\gamma$: and $c = -\frac{2}{5}(\alpha+\beta+\gamma+\ldots)$ where $\alpha = \frac{1}{256}$, $\beta = \frac{1}{4}.\frac{3}{1}.\frac{7}{6}\alpha$, $\gamma = \frac{1}{4}.\frac{5}{2}.\frac{9}{7}\beta, \ldots$; but in all these series it is obvious that the ratio of the terms, as they diminish, approaches to equality; so that it is even difficult to determine whether or no this sum is finite. But a still greater objection to this method is, that the third fluxion $-8S_3\varsigma x$, treated in the same way, affords a very different result.

Simple pendulum with resistance as the square of the velocity.

Scholium 2. In the case of a simple pendulum, subjected for a single vibration to a resistance proportional to the square of the velocity, the space described may be correctly calculated by means of a logarithmic equation, and the time might also be expressed, if it were required, in a series. Let the space described be x, and the resistance y, then the force may be represented by $1-x-y$, and the square of the velocity will be $\int(\dot{x}-x\dot{x}-y\dot{x})$, whence $y = a\int(\dot{x}-x\dot{x}-y\dot{x})$, and $\dot{y} = a\dot{x} - ax\dot{x} - ay\dot{x}$, and $\dot{y} + ay\dot{x}$ $= a$

$=(a-ax)\dot{x}$. Then if we make $y=uz$, z being a function of x, and $\dot{y}=u\dot{z}+z\dot{u}$, we have $u\dot{z}+z\dot{u}+auz\dot{x}=(a-ax)\dot{x}$; and in order to determine z, we may put $u\dot{z}+auz\dot{x}=o$, $\frac{\dot{z}}{z}+a\dot{x}=o$, HL$z=-ax$, or $z=e^{-ax}$, and, substituting for $z\dot{u}$, $e^{-ax}\dot{u}=(a-ax)\dot{x}$, or, if $ax=w$, $e^{-w}\dot{u}=\dot{w}-\frac{w}{a}\dot{w}$, $u=e^{w}\dot{w}-\frac{1}{a}e^{w}w\dot{w}$, and $y=e^{-w}\int e^{w}\dot{w}-\frac{1}{a}e^{-w}\int e^{w}w\dot{w}$. Hence (Eul. calc. integr. pr. 52) $y=Ce^{-w}+1-\frac{1}{a}De^{-w}-\frac{1}{a}w+\frac{1}{a}=Ce^{-ax}+1-\frac{1}{a}De^{-ax}-x+\frac{1}{a}$, which must vanish when $x=o$, and $e^{-ax}=1$, whence $C-\frac{1}{a}D=-\frac{a+1}{a}$, and $y=\frac{a+1}{a}(1-e^{-ax})-x$.

Scholium 3. The same result may be still more simply obtained, by repeated approximations, from the first expression $y=a\int(\dot{x}-x\dot{x}-y\dot{x})$: neglecting first the term $y\dot{x}$, we have $y=ax-\frac{1}{2}ax^2$, then substituting this value in $y\dot{x}$, we have $a\int y\dot{x}=\frac{1}{2}a^2x^2-\frac{1}{2\times3}a^2x^3$, and $y=ax-\frac{1}{2}ax^2-\frac{1}{2}a^2x^2+\frac{1}{2\times3}a^2x^3$, to which we add, by another similar operation, the terms $+\frac{1}{2\times3}a^3x^3-\frac{1}{2.3.4}a^3x^4$, and the whole becomes $ax-\frac{1}{2}a^2x^2+\frac{1}{2\times3}a^3x^3-\frac{1}{2.3.4}a^4x^4\ldots-\frac{1}{2}ax^2+\frac{1}{2\times3}a^2x^3-\ldots$ But it is well known that $e^{-ax}=1-ax+\frac{1}{2}a^2x^2-\frac{1}{2\times3}a^3x^3\ldots$, therefore, $y=1-e^{-ax}-\frac{1}{a}e^{-ax}+\frac{1}{a}-x$; precisely as before. The length of the whole vibration may obviously be found by making $y=0$: and if it be desired to determine the time, we must develope, by means of the polynomial theorem, the expression $\frac{\dot{x}}{a}(ax-\frac{1}{2}(a+a^2)x^2+\frac{1}{2\times3}(a^2+a^3)x^3-\ldots)^{-\frac{1}{2}}$, and take the fluent. — Different investigation.

Scholium 4. This mode of exhaustion may be illustrated by another well-known case of a pendulum with a constant resistance, which is known not to alter the time of vibration. Calling the initial force 1, and the resistance a, the velocity destroyed will be ax, if x be the arc proportional to the time; and the diminution of the space will be $\frac{1}{2}ax^2$: but this displacement will cause a new force, which is to the initial force 1 as $\frac{1}{2}axt^2$ to the radius 1, and is therefore represented by $\frac{1}{2}ax^2$, hence the velocity will be increased by the quantity $\int\frac{1}{2}ax^2\dot{x}=\frac{1}{2\times3}ax^3$, and the space by $\frac{1}{2.3.4}ax^4$: so that we have $\frac{1}{2}ax^2-\frac{1}{2.3.4}ax^4$ for the corrected — Pendulum with constant resistance.

corrected displacement : and this correction will, in a similar manner, afford a second of $\frac{1}{2..6}ax^6$; so that the true displacement becomes $\frac{1}{2}ax^2-\frac{1}{2..4}ax^4+\frac{1}{2..6}ax^6-\frac{1}{2..8}ax^8-\ldots$ which, as is well known, is equal to $a-a\varsigma x$: and the diminution of the velocity $ax-\frac{1}{2\times 3}ax^3+\frac{1}{2..5}ax^5-.. = aSx$; which will, of course, vanish, when $x=180°$; so that the body will be at rest at the expiration of the corresponding time of a complete vibration in one direction. And a similar mode of calculation may be applied to the case of a simple pendulum, with a resistance varying as the square of the velocity, except that here the variation of the resistance at each step makes the process more complicated.

Theorem D.

Peculiarities of compound vibrations with resistance.

If the resistance be proportional to the square of the velocity, a pendulum, of which the point of suspension performs vibrations composed of two regular vibrations, may have its greatest excursions a little after the greatest excursions of the point of suspension when its vibrations are inverted, and a little before them when they are direct, provided that the slower vibrations be the larger.

In order to express the resistance as correctly as possible in this case by a series of multiple arcs, it would be necessary to have a great variety of terms, some approaching in their periods to the primitive vibrations, others triple and quintuple of these : but for the present purpose these greater multiples may be safely omitted, taking care only that the omission do not affect the determination of the coefficients of the rest. The general methods of obtaining a series in the terms of sines and cosines of multiple arcs fail here, as before, on account of the positive terms resulting from the squares of negative quantities, where the conditions of the problem require that they should be negative, and it is necessary to employ approximations obtained from the results of individual substitutions. For this purpose a series of five or six terms has been tried in various ways without success : and the most convenient form which has been discovered consists of three only, two isochronous with the primitive vibrations,

vibrations, and the third having a recurrence less frequent by one time in the common period than the slowest of these: then the coincidence being established at the time of the greatest and least excursions, and at the transit of the vertical line nearest to the middle of the intermediate time, a mean value of the coefficients may be obtained, which no where differs very materially from the truth; although, if we desire to make the coincidence more perfect in any given part of the period, we may do it by altering the values of the coefficients a little; and by these means we may obtain a correction of the approximation, sufficiently near to the truth. We may also suppose the actual compound vibrations to preserve their regularity without any material deviation, following the same law as if the resistance were either inconsiderable, or varied simply as the velocity; and we may make the proportion of the greatest to the least actual vibration that of $m+1$ to $m-1$; then calling the periodical time of the greater primitive vibration (☽) t, that of the lesser (☉) being unity, and x being the arc corresponding to the time in the latter, beginning with the perfect coincidence in the vertical line, the distance from that line at any subsequent time will be expressed by $Sx+m\,S\frac{x}{t}$; and the velocity by $\varsigma x+\frac{m}{t}\varsigma\frac{x}{t}$, creating a resistance which may be called $r\left(\varsigma x+\frac{m}{t}\varsigma\frac{x}{t}\right)^2$, which has already produced a displacement determinable as in the former proposition, whence we may obtain from the true place the place in which the body would have been found if there had been no resistance. In order to facilitate the computation, we may assume particular values of m and t, making the one 3, and the other $1\frac{1}{29}$; and then determine the coefficients of the formula $a\varsigma x+b\varsigma\frac{28}{30}x+c\frac{28}{30}\varsigma x=(\varsigma x+2{\cdot}9\varsigma\frac{28}{30}x)^2$, so as to obtain as correct a coincidence as possible of the magnitude and of the period of the joint vibrations at the time more immediately to be considered. Now it is easily shown, from the well known properties of compound vibrations, as applied to the intervals of successive spring and neap tides, that the interval between two of the greatest vibrations will be expressed very nearly by $\frac{m+1}{m+t}t\,360^\circ$, and the interval between two of the smallest by $\frac{m-1}{m-t}t\,360^\circ$, provided that the periods differ but little from each other: and from these formulas we must determine the proportions of the coefficients

a and

a and c; for b must always be $1+2{\cdot}9^2=9{\cdot}41$, in order that the equation may hold good for the greatest and least vibrations: consequently $a+c=5{\cdot}8$. We may first allow $\frac{9\cdot41}{3}$ to a, in order to form with b a result similar to the true compound vibration, and the remainder $2{\cdot}663$ must again be distributed between a and c in such a proportion that the interval of the greatest vibrations may be $\frac{40}{39}\,360^\circ$, and m must be so determined for this purpose, that $\frac{m+1}{m+\frac{30}{28}}\cdot\frac{30}{28}$ shall be equal to $\frac{40}{39}$, whence $m=\frac{3}{5}$, and for this part of a, $'a:c=1:\frac{3}{5}$, $'a:a+c=1:\frac{8}{5}$, and $'a=\frac{5}{8}.2{\cdot}663=1{\cdot}664$, $a=4{\cdot}80$, and $c=1$, so that the velocity becomes $4{\cdot}8\varsigma x+9{\cdot}41\varsigma\frac{29}{30}x+\varsigma\frac{28}{30}x$: and for the interval of the least vibrations, $\frac{m+1}{m+\frac{30}{29}}\cdot\frac{30}{29}=\frac{20}{19}$, and the whole is found $3{\cdot}8\varsigma x+9{\cdot}41\varsigma\frac{29}{30}x+2\varsigma\frac{28}{30}x$; and for a mean value of the coefficients, $4{\cdot}3\varsigma x+9{\cdot}41\varsigma\frac{29}{30}x+1{\cdot}5\varsigma\frac{28}{30}x$.

Determination of the greatest excursion.

If now we denote the ratio of the square of the time of the most frequent vibration ⊙ to that of the square of the time of the spontaneous vibration of the pendulum (♒) by the ratio of n to 1, the corresponding displacement will be to the distance expressive of the force as $\frac{n}{n-1}$ to 1, and the term $4{\cdot}8\varsigma x$ will exhibit a displacement of $\frac{4\cdot8n}{n-1}\varsigma x$: and in the other terms, substituting, for n, $(\frac{30}{29})^2 n$ and $(\frac{30}{28})^2 n$ respectively, we have $\frac{1\cdot07n}{1\cdot07n-1}$ and $\frac{1\cdot145n}{1\cdot145n-1}$, or $\frac{n}{n-\cdot9344}$, and $\frac{n}{n-\cdot871}$ for multipliers; and the distance thus determined shows the place in which the body would have been if there had been no resistance, which is before the true place when the multiplier is positive, and behind when it is negative: the distance of this virtual place therefore becomes from $Sx+3S\frac{29}{30}x$, $Sx+3S\frac{29}{30}x+rn\left(\frac{4\cdot8}{n-1}\varsigma x+\frac{9\cdot41}{n-\cdot9344}\varsigma\frac{29}{30}x+\frac{1}{n-\cdot871}\varsigma\frac{28}{30}x\right)$, and the velocity $\varsigma x+2{\cdot}9\varsigma\frac{29}{30}x-rn\left(\frac{4\cdot8}{n-1}Sx+\frac{29}{30}\cdot\frac{9\cdot41}{n-\cdot9344}\varsigma\frac{29}{30}x+\frac{28}{30}\cdot\frac{1}{n-\cdot871}\varsigma\frac{28}{30}x\right)$. Here it is obvious that as n approaches to 1, to ·9344 or to ·871, the value of the corresponding term increases without limit, and the period of the resistance may approach to that of the slower vibration, or may even exceed it, in very particular circumstances:

stances: and if these periods were equal, the effect would be the same as if the whole resistance were attached to the slower vibration, which would obviously be such as is stated in the theorem. But for a more particular illustration, we may take $n=\frac{2}{3}$, and $r=\frac{1}{20}$: the distance of the virtual place will then become $Sx+3S\frac{29}{30}x-\cdot48\text{ç}x-1\cdot103\,\text{ç}\frac{29}{30}x-\cdot163\text{ç}\frac{28}{30}x$: and by substituting in this formula a number of different values for x, we find, when $x=118°$,—252°, and—623°, maxima amounting to 4.353, 4·367, and 4·342 respectively; and, employing the other values of a and c, a maximum of 2·055 when $x=15\times360°-280°$. Here it is obvious that the maximum for the virtual place is anterior to the true maximum, the excursion 4·367 being considerably greater than 4·353, which is nearest to the true maximum; or, in other words, the true maximum happens a little after the perfect conjunction of the forces which occasion it, which if there were no resistance, would coincide with the maximum of the excursions.

THEOREM E.

Disturbances from gravitation.

The disturbing force of a distant attractive body, urging a particle of a fluid in the direction of the surface of a sphere, varies as the sine of twice the altitude of the body.—*See Nicholson's Journal, XX.* 209.

THEOREM F.

Equilibrium of a particle on a spheroid.

The inclination of the surface of a spheroid, slightly elliptical, to that of the inscribed sphere, varies as the sine of twice the distance from the circle of contact; and a particle resting on any part of it without friction may be held in equilibrium by the attraction of a distant body.—*See Nicholson's Journal, XX.* 209.

Corrollary. Hence it may be calculated, neglecting the density of the sea, that the primitive solar side would be ·807, and the lunar 2·0166 feet, supposing the lunar disturbing force to the solar as 5 to 2.

THEOREM G.

Equilibrium of a spheroid.

The disturbing attraction of the thin shell, contained between

a spheroidical

a spheroidical surface and its inscribed sphere, varies in the same proportion as the inclination of the surface, and is to the relative force of gravity, depending on that inclination, as 3 times the density of the shell to 5 times that of the sphere.—*See Nicholson's Journal, XX.* 210. 213.

Tides.

Corollary. Hence the ellipticity must be to that which would take place if the density n of the sphere were infinite, as 1 to $1-\frac{3}{5n}$; or, in the case of $n=5\frac{1}{2}$, nearly as 8 to 9, giving for the solar tide ·91, and for the lunar 2·263 : if $n=5$, the heights are ·92 and 2·291 respectively; if $n=1$, 2·024 and 5·042.

Length of the pendulum.

Scholium. The direct attraction, determining the length of the pendulum in different latitudes, may be calculated in a manner nearly similar.—*See Nicholson's Journal, XX.* 273.

Theorem H.

Propagation of waves.

When the horizontal surface of a liquid is elevated or depressed a little at a given point, the effect will be propagated in the manner of a wave, with a velocity equal to that of a heavy body which has fallen through a space equal to half the depth of the fluid, the form of the wave remaining similar to that of the original elevation or depression.—*See Lagrange, Mécanique Analitique; or Young's Natural Philosophy, II.* 63.

Scholium. In calculating this velocity, it would probably be more correct to diminish the multiplier about $\frac{1}{16}$ or $\frac{1}{8}$, as is found to be necessary for determining the velocity of the motions of fluids in most other cases. (*See Philosophical Transactions,* 1808.)

Theorem I.

Reflection of waves.

A wave of a symmetrical form, with a depression equal and similar to its elevation, striking against a solid vertical obstacle, will be reflected so as to cause a part of the surface, at the distance of one-fourth of its breadth, to remain at rest; and if there be another opposite obstacle at twice that distance, there may be a perpetual vibration between the surfaces, the middle point having no vertical motion. — *See Young's Natural Philosophy, I.* 289, 777.

Thus

Thus the vibrations of the water supposed to be contained in a canal in the situation of the equator, and 90° in length, would be synchronous with the passage of a wave 180 in breadth, over any point of a canal of the same depth: and the elevation and depression of a spheroid, compared with the mean height, exhibits a symmetrical wave in the sense of the proposition.

(To be continued.)

II.

An explanatory Statement of the Notions or Principles upon which the systematic Arrangement is founded, which was adopted as the Basis of an Essay on Chemical Nomenclature. By Professor J. BERZELIUS.

(Concluded from p. 131.)

A PORTION of the solution of the oxide in caustic kali, was precipitated by alcohol, in order to obtain the combination, if possible, in a state of purity. I dried the precipitate in a press between blotting paper, and then heated it to a red heat. Four grammes of the heated mass, (which, previous to exposure to the fire, did not contain any carbonic acid) were treated with the muriatic acid, which caused a strong effervescence to take place, and took up the kali, leaving the oxide untouched. I obtained 0·7 grammes of muriate of kali, and 3·4 grammes of the oxide, which, consequently, had been combined with 0·44 grammes of kali—that is to say, the oxide contained, in this combination, ten times as much oxigen as kali. But it is probable that this experiment cannot be relied upon with much confidence. Oxides of tin.

A solution of the caustic kali, much diluted with water, and digested with more of the oxide of tin at a maximum than it could dissolve, was evaporated to dryness, and treated with nitric acid. It yielded 193·2 parts of the red oxide of tin, and 12 parts of potash. The first contained 42 parts of oxigen, and the latter 2·04—that is to say, the oxide contained twenty times as much oxigen as the potash. But I repeat, that when the affinities of bodies are too weak, and the difficulty of attaining

Oxides of tin. taining the just point of saturation considerably great, these sort of analytical experiments are not to be confided in.

The following experiments, perhaps, deserve a little more confidence. I prepared a saturated solution of the oxide of tin in caustic kali, which was precipitated by means of water of barytes. The precipitate was white, flaky, and very voluminous. I washed and dried it very quickly. A part, treated while still wet, with muriatic acid, was decomposed without effervescence; another part, dried and heated to a red heat, in a platina crucible, became of a fine lemon colour, and, when treated with muriatic acid, it produced a strong effervescence. Four grammes of this red mass produced three grs. of the oxide, and 0·79 grs. of barytes. The four grammes which are wanting, is carbonic acid, and corresponds almost exactly with the barytes. The oxide contains 65·4 parts of oxigen, 8·3 parts of barytes—that is to say, the first contains eight times as much as the latter. The mixture of the carbonate of barytes, and the oxide of tin, at red heat, preserved even in the highest temperature that I could give it, its yellow colour, probably because the carbonic acid prevents the action of the alkaline earth on the oxide, by means of which it would have been reduced to the intermediate degree; but, at the points where the mixture had been in contact with the carbon, it had become white.

Lime-water precipitated the stannate of kali in the same manner as the water of barytes. The stannate of lime readily attracted the carbonic acid of the air; it turned yellow at a cherry red heat, and soon became white, without the influence of any combustible body when it was exposed to a bright red heat.

The stannate of kali, used in order to precipitate the metallic solutions, produced the following results:

In the plumbic nitrate, it gave a white precipitate, which afforded a small quantity of water when at a red heat, and was changed to a straw colour.

In the cobaltic muriate, it gave a blueish precipitate, which, when washed in boiling water, became red; and, by drying, changes to a deep brown, and of a vitreous fracture. At a red heat it loses water, and turns black, giving to paper the colour of of umber. At a very high temperature it becomes an azure blue, and when pulverized, does not lose its colour. Oxides of tin.

In the nitrate of copper, it gave a greenish white precipitate, which, after drying, had a vitreous fracture, and was of a saturated green colour. When at a red heat, it became black, and gave to paper a greenish umber colour.

In the sulphate of manganese, this white precipitate turns brown when in contact with the air.

In the ferreous sulphate, it gave a white precipitate which in contact with the air, assumed all the colours belonging to the oxides of iron, and terminates by becoming a vitreous mass, of a deep brown colour, from which the oxide of iron cannot be extracted, except to a certain degree, by the muriatic acid. By dissolving impure tin in the nitro-muriatic acid, this same combination is obtained, which is precipitated during the evaporation of the acid, in the form of a blueish green powder, and which, after being exposed for some days to the access of the air, assumes a dark rusty colour.

In the hydrargric muriate, (corrosive sublimate) it gives a yellow precipitate, which, after some moments, becomes red—when well washed, it turned carnation, and, after being dried, it was of a brown colour, and appeared to be a triple combination.

In a solution of the muriate of ammonia, it gave a white precipitate, soluble by the addition of a certain quantity of water—and this solution was easily precipitated by adding ammonia. The precipitate collected on the filtre is readily dissolved in water, after the alkaline liquor has been drained off. This solution, left in an open vessel, becomes, after a few days, less fluid, and nearly like a saturated solution of gum arabic, without losing any of its transparency. I attribute this to a commencement of deposition of the oxide.

A solution of the oxide of tin in a diluted lixivium of caustic kali being left in a closed phial, was decomposed by degrees, the oxide was precipitated, and the fluid remained like a jelly. This jelly, on being shaken a little, became liquid, and the oxide passed through the filtre—if they are to be separated, it requires some days for the jelly to set again.

These experiments prove, that the oxide of tin, although it possesses some of the properties of an acid, has nevertheless so

Oxides of tin. weak an affinity to the saline bases, that its combinations with them are decomposed by circumstances which do not seem to have any influence on the other saline combinations. We have seen, that the stannate of manganese is decomposed by the access of air, and that the alkaline and earthy stannates are decomposed both by the air and by fire.

I think, therefore, that, notwithstanding the great analogy which exists between the oxides of this, and those of antimony, they ought not, in preference, to be called acids, because their combinations, as saline bases, have a much longer duration than those in which they appear as an acid. But tin possesses the singular property of producing three salifiable oxides—that is, one degree of oxidation more than the nomenclature admits of. I take the liberty of giving the latter of these three degrees of oxidation the termination *eum* with the name of the metal; I therefore call them *oxidum* stannosum, oxidum stannicum, and oxidum stanneum. Their combinations with the acids I shall call, for example, murias stannosus, murias stannicus, and murias stanneus. It is evident, that the nomenclature allows of their combinations with the saline bases, being called stannates and stannites, without its being necessary to give the name of acid to these oxides in their isolated state.

The Combinations of Tin with Sulphur.

Various chemists have employed themselves in examining the sulphurets of tin, and their results have been frequently very different; the reason is, that tin requires, in order to unite with sulphur, a temperature sufficiently high to volatilize the greatest part of the sulphur before the combination takes place, and consequently there remains, at that moment, too small a quantity to saturate the metal. On analysing the imperfect combination, the results were, as may be supposed, very variable. It appeared, that the same thing took place with the sulphurets of other metals. The celebrated Vauquelin has published a table of the composition of sulphurets, which too clearly proves the truth of what I have said.

I have already mentioned, that I had prepared a saturated sulphuret of tin, on remelting the pulverized sulphuret, and carefully mixing it with pulverized sulphur, it formed, on this occasion, first,

first, a sulphuret, which was decomposed at a temperature sufficient to melt it, so that the melted mass consisted of a saturated sulphuret of tin*. Oxides of tin.

The undermentioned experiment, in which the sulphuret of tin produced an almost equal quantity of oxide of tin at a maximum, proves that the relation between the oxides and the sulphuret of this metal, is precisely the same as that which I found long ago with regard to lead—that is to say, the sulphuret of tin must be composed as follows :

Tin	79·6	100·000
Sulphur . .	21·4	27·234

Proust, in a very interesting memoir, has endeavoured to prove, that aurum musivum is a combination of sulphur with the oxide of tin, but oxided to a degree which does not exist in any other case, and he has adopted his conclusion from the circumstance, that this preparation, decomposed by fire in a closed vessel, produces sulphur, sulphuret of tin, and sulphureous acid; and I myself have actually verified, that aurum musivum, prepared in the usual manner, always produces, on this occasion, a small quantity of sulphureous acid. I have very just reasons, however, to consider this sulphureous acid as the product of matters different from the composition of aurum musivum.

Three grammes of sulphuret of tin, reduced to fine powder, and mixed with 1½ grammes of pure sulphur, were exposed to a cherry red heat in a little glass ball with a narrow neck, closed at the mouth by a piece of charcoal. The ball was half filled. When the excess of sulphur had passed off, I took it out of the fire. The three grammes now weighed 3·33 grammes, and was converted into a greyish yellow mass, which, except the deep colour, had all the characters of aurum musivum, which, in consequence of the method of its preparation, cannot contain oxigen. I considered it, at first, as incomplete aurum musivum, which, if heated more with sul-

* I tried, before I found this method, several others, which answered my purpose very imperfectly. Amongst others, I made an amalgam of five parts of tin with one part of mercury. I then reduced them to powder, and mixed them with sulphur. This mixture I exposed to the fire in a glass phial. The combination was made instantaneously, and broke the phial with an explosion.

 phur,

Oxides of tin. phur, would acquire the characters of the most perfect aurum musivum. On taking it out of the ball, I mixed a fresh quantity of pulverized sulphur, and heated it in the same ball again; but it received no addition of weight. On a repetition of the experiment, the result was the same. In these experiments 100 p. of sulphuret of tin absorbed 11 p. of sulphur, in order to produce a species of aurum musivum, which nevertheless differed from the common sort, in its colour being less brilliant, and more inclining to grey. But these 11 p. of sulphur are, with a very slight difference, half as much as the sulphuret contained before; and we have here an example of sulphuration, which contains $1\frac{1}{2}$ as much sulphur as the preceding one. I do not yet know any other example of this multiplier ($1\frac{1}{2}$) in the sulphurets. I made several vain attempts to combine this sulphuret with more sulphur by heat, and that proved to me still more, that the body I had obtained was a degree of the sulphuret of tin, of which no mention has been made by chemical authors. This degree of sulphuration is composed in the following manner:

Tin	71·8	100·000
Sulphur . .	28·2	40·851

Aurum musivum, or mosaic gold. It is very difficult to say what circumstance most principally contributes to the formation of the mosaic gold in the common manner of preparing it; and it is very difficult to obtain a mosaic gold in this manner perfectly saturated with sulphur, and deprived of all foreign matters. We cannot consider any other aurum musivum as pure, except that which has been sublimed in bright yellow crystals during the operation; but of this the quantity is usually very small. If we endeavour to sublime aurum musivum which has already been prepared, it is decomposed, and the sulphur is obtained, and the usual sulphuret remains. A large piece of this aurum musivum was slowly decomposed in a glass retort by a moderate heat, which was not sufficiently strong to melt it; and when the mass was half decomposed, I took the retort from the fire. I found in the remaining mass three different layers. The first was porous, grey, and metallic—it was the usual sulphuret. The second, which was less porous, and of a greyish yellow, consisted of the intermediate sulphuret which I have described. This layer was very fine, and of not more

more than half a line in thickness. The last layer contained the aurum musivum, which had undergone no change. This proves, that the aurum musivum, before it is reduced to the common sulphuret, passes through the intermediate degree; but, as this last layer is very fine, it is evident, that the difference of temperature which it requires to produce these two inferior layers, is very trifling; and when the fire has been a little too strong, any traces of the intermediate layer are very seldom seen. Oxides of tin.

The intermediate sulphuret of tin, digested with concentrated muriatic acid, produces sulphurated hydrogen, which, by degrees, assumes a fine yellow, and becomes aurum musivum, which remains insoluble, and is obtained by filtering the liquor. This decomposition takes place very slowly, but even after some hours of digestion, the sulphurated hydrogen gas continues to be disengaged, though still decreasing in quantity.

I have made several attempts to analyse the aurum musivum, but I have not been able to procure this combination in a sufficiently pure state, and perfectly separated from the intermediate sulphuret. A portion of the common aurum musivum, which did not contain cinnabar, a thing this preparation is frequently adulterated with, in consequence of the mode in which it is prepared, was decomposed by means of the nitro-muriatic acid. This liquid, precipitated with the muriate of barytes, produced three grammes of aurum musivum, 7·463 grs. of sulphate of barytes, ignited in the fire, which correspond with 1·03 grs. of sulphur. Therefore it follows, that 100 parts of tin had been combined with 52·3 parts of sulphur. This is not exactly double what the tin contains in the common sulphuret, but the difference is not very considerable, and, with the confirmation which the doctrine of multiple proportions has already gained, I think we may determine, with some degree of certainty, that aurum musivum contains twice as much sulphur with the same quantity of metal as the common sulphuret.

It is very evident, that oxigen is not necessary to the constitution of aurum musivium, because the latter is produced by the action of muriatic acid on the intermediate sulphuret; on which occasion no other oxidation can be imagined, except that which produces a muriate of tin.

As to the nomenclature of these three degrees of sulphurets,

it

Oxides of tin. it is very plain, that which I have proposed is insufficient; and it is necessary to suppose a more correct principle, or one that is capable of being carried to a greater extent. I have reason to suppose, that all bodies may be combined to a certain number of multiples, which are the same for all. For example, all combustibles are capable of an equal number of degrees of oxidation, as well as of sulphuration. In this case, the best principle for the nomenclature of sulphurets would be the addition of the number of the multiple proportion in which the sulphur is combined with the metal, as, for example, disulphuretum, trisulphuretum, &c. It is yet too early to apply this principle to sulphurets, because we are still ignorant where we should commence our account. Nevertheless, by taking the common sulphuret of a metal, that is to say, the highest degree of sulphuration which, in a closed vessel, may be heated to a red heat, without being decomposed. Taking this, I repeat, as an artificial point from whence we may begin to count, we shall have for the names of these three sulphurets of tin, (*a*) sulphuretum, (*b*) sesquisulphuretum, and (*c*) disulphuretum stanni. Although it may be offered as an objection, that the second of these denominations involves an incorrect notion. With respect to the atomistic theory, I must observe, that no theory has been assumed as the basis of my experiments on determinate proportions; but nevertheless it appears, that the result of all my experiments on this object will confirm such a theory. It will be the time to amend this pretended incorrectness in the nomenclature when the theory illustrated by ulterior experiments shall have proved to us which of these combinations may be composed of an equal number of atoms of the two constituents, and which consequently ought to be the first. Until this is decided, I think that the name sesquisulphuretum will be sufficient for our purpose as a denomination.

III.

Observations respecting the Figure of a drowned Man formed in the Ice of the Pond in Halnaker Park, and upon the Explanations which have been offered of that singular Phenomenon. In a Letter from Sir George Cayley, *Bart.*

To Mr. Nicholson.

Brompton, May 29, 1813.

SIR,

IN No. 159 of the Chemical Journal, I observe an ingenious paper of your's respecting the singular phenomenon of the figure of a drowned person being exhibited in the ice above him. I have read with attention the very ingenious explananation you have given of this fact; and it may, perhaps, be the true mode of accounting for it; but I am not perfectly satisfied with your theory, and wish to draw your attention to another cause that may have operated in producing this appearance, and which I think worthy of investigation by a simple experiment in the same pond next winter, through the medium of any gentleman residing near it. Whether the figure on the ice be well accounted for.

You must frequently have observed, that in muddy ponds a great quantity of gas (carburetted hydrogen) rises in bubbles from the bottom, and that, although this effect is produced with greater rapidity in summer than in winter, yet that pond ice is very frequently quite full of bubbles and blisters from this source, some of which are very minute, and others forming large flat circles resembling smooth shillings or half crowns within the ice at different depths, according to the thickness it possessed when the bubble ascended. Carburetted hydrogen rises in ponds.

Water parts with the small portion of air it held in solution during freezing, and thus a number of minute bubbles are found in almost all ice; but the copious supply of bubbles I allude to, certainly arise from the bottom, as the portion of water congealed at the surface is not sufficiently large to produce them. If, under these circumstances, any convex body rested lightly upon the mud, as that of the drowned person alluded to, all the pond would become affected with the bubbles, excepting that portion of it immediately above the body, where The large bubbles in ice supposed to arise from the bottom, and liable to be intercepted by any substance lying there. Hence the theory of the figure.

where they would be intercepted by it, and obliged to pass obliquely under it on every side, till they could rise with perfect perpendicularity, and thus in the act of freezing, give an accumulated whiteness to a perfectly distinct outline of the figure in the ice, as appears to have been the case in this instance. The ice forming within this outline would not be agitated, and therefore would be clear, hard, and slippery, according to the reasons you have given, and the test of universal experience, whereas the whole remaining ice would be full of white bubbles, and the confused crystallization would include the impurities of the water. The chief objection to this theory is, that the figure of the hat should have been formed, as well as that of the body; but if the delineation be correct, it appears to me, that the whole length of the figure with the hat on is given; and that the head, resting on the back of the hat, has formed the thick appearance represented, aided, perhaps, by some derangement of the frock. The objections that occur to me respecting your theory, are—first, that there was scarcely time for putrefaction to have commenced so as to have generated any heat. Secondly, that if such hot streams had risen, they would probably have spread on all sides under the ice, leaving no abrupt outline, much less an accumulated one. And thirdly, that if heat confined to this space had been the cause, the ice must have been thinner over the body than in other parts of the pond, which was not the case. On the supposition of bubbles from the bottom being the cause of the phenomenon, the precision of the figure is satisfactorily accounted for, excepting the alleged want of the hat, which, if it be not a mistake, militates equally against the other theory, as no good reason can be given why the wet hat should be a worse conductor of heat, than the rest of the wet clothing, particularly where consisting of many folds. The plate expresses a greater breadth of outline at those places where the figure is the thickest, and this would be a necessary consequence of bubbles, as their accumulation at any point must correspond with the magnitude of surface at the bottom, intersected by that particular part of the figure. Upon the whole, therefore, I am inclined to think, that this curious fact is more likely to have been caused by bubbles than by heat, although if it could be clearly made out that there was no farther representation of the head than

Objections to the Editor's theory of putrefaction in the body, &c.

than to the face, this theory must be given up; unless, indeed, the top of the head had been forced under the mud, which is not very probable.

I think it very possible that this *ice ghost* may be put to rest, if any person will place a convex piece of wood, weighted to be rather heavier than water, at the bottom of the same pond next winter, when, perhaps, its figure may become painted on the ice in a similar manner to that of the unfortunate person drowned there.

Proposed experiment to place a piece of wood at the bottom of the pond.

The fact of the thin covering of snow having been dissolved upon the clear ice, and retained upon the rough, may possibly be owing to the different conducting powers of ice in these two states. I have frequently observed, that hoar frost continues longer over hollow drains than in other places, which I conceive to be owing to the communication of heat from the body of the earth being intercepted by the confined air. The bubbles, extraneous matters, and confused crystals, of the rough ice, may be less capable of conducting caloric, than the compact clear ice; hence, when the temperature of the air was below the freezing point many degrees, the snow upon the clear ice might be raised nearer to the temperature of 32, or that of the water below, than that upon the rough ice, where the water from below had less influence. Under these circumstances, the sunshine, or other transient warmth, may have created a temporary thaw, which has been sufficient to melt that thin portion of snow already but little above the freezing point, whereas it might not be able, during the same interval of time, to raise the other portion of snow from a low temperature up to the freezing point, and to dissolve it also. I must acknowledge, that it requires a very nice adjustment of all these circumstances to have produced the effect described, and therefore that less confidence can be placed in this theory of the fact; but whatever be the true account, it seems probable that some unusual combination of circumstances must have taken place; and therefore it becomes more justifiable to point out one at least of the possible combinations capable of producing the event.

The absence of snow on the ice of the figure deduced from difference of conducting power.

I remain, Sir,
Your obliged and obedient Servant,
GEORGE CAYLEY.

P. S.

The theory becomes more difficult from the remarkable fact of bog timber being delineated upon ice.

P. S.—Since the above was written, I have seen the curious observations of Mr. Harvey, and likewise the very extraordinary fact of trees being discovered under the Irish bogs by a corresponding delineation upon the surface of the ice above them, as stated by Dr. Chichester. This fact, as it seems to occur frequently, deserves to be fully investigated by competent persons. Had these trees only been at the bottom of the water, and not *within* the bog, I should have ascribed their appearance on the ice either to a generation of bubbles from the tree, rising to the surface in the shape of the branches, or by their having intercepted the bubbles rising from the bottom of the bog, when they generated no bubbles; thus varying the conducting power of the ice, and affecting its attractive influence towards the deposition of the hoar frost. But I suspect from this fact that there is something more extraordinary in these ice figures than the ascending streams of heated water, oleagenous particles, or bubbles, will account for.

IV.

Description of a Machine for reaping, used anciently by the Gauls.*

Great advantages to be derived from an extended publication of useful machines, &c.

THAT nothing should be neglected which relates to the arts, but that all the known processes ought to be described with care and precision, has been incessantly repeated. M. Lenormand, Professor of Natural Philosophy and Chemistry, and the author of various memoires, expressed his persuasion, that the arts would have made a more rapid progress if a collection had been long ago made to form a work containing all the inventions which have been known. In such a general repertory, where every one could refer to that which might be most useful to his pursuits, he thinks the most inferior machine, or the most indifferent process, might give birth to some valuable conclusions in the minds of such ingenious individuals, as are endowed by nature with the talent of inventing, or of bringing to perfection, discoveries already made. Impressed with this conviction, he has thought it incumbent on him not

* Annales des Arts, XL. 138.

to

to overlook the description of an ingenious machine, known to very few persons, and which we are persuaded few learned men have remarked.

Passage in P. R. T. Æmilianus, describing an ancient reaping machine of the Gauls.

"Within these few days," says M. Lenormand, "one of my friends, who is much employed in reading the ancient authors, was struck with a passage of *Palladius Rutilius Taurus Æmilianus*, in which he describes a machine used amongst the Gauls for reaping their fields. The construction of this machine considerably embarrassed my friend, who was not well acquainted with mechanics. Knowing that I occasionally made them my study, he brought me that author, and requested me to explain, as well as I was able, the detail which he gave. He farther wished me to make a drawing of this machine if I succeeded in clearly understanding it.

Drawing and explanation developed.

"I think I have understood the meaning of the author, and have drawn the machine according to his description; and I think the publication of it may be of use in assisting the progress of the arts.

Time at which this author wrote.

"It is well known, that the precise epocha at which our author wrote, cannot be determined. It is ascertained, that he lived before Cassiodorus—that is to say, before the year 514 of the vulgar era, after the decay of letters in Rome. Palladius was still living in the fifth century, and the reader will be not a little surprised to find at this epocha, which is termed by the moderns the *barbarous ages*, that the arts had arrived at such perfection, and the description of this machine given by our author with such correctness and precision.

The machine, though not perfect, indicates an advanced state of the arts.

"I do not pretend to say here, that this machine was perfect. Much is wanting—but though its construction shews many imperfections, we ought not the less to conclude, that even at that period, men were already occupied with the improvement of the arts, for this machine was undoubtedly not the only one in use.

The passage translated.

"The following is a literal translation of that passage of the author*.

* Pars Galliarum planior hoc compendio utitur ad metendum, et præter hominum labores, unius bovis opera spatium totius messis absumit. Fit itaque vehiculum, quod duabus rotis brevibus fertur; hujus quadrata superficies tabulis munitur, quæ forinsecùs reclives in

"'In

Reaping carriage used by the Gauls in the plain country, described.

"'In that part of Gaul where there are plains, the inhabitants have a method of reaping, which greatly economises the labour of men, and by which a single ox can get in all the harvest. For this purpose, they make use of a machine drawn upon two small wheels, the square surface of which is furnished with planks inclining outward, in such a manner thattheyrender the upper part much larger than the lower. The plank in front is not so high as the others. On this board are placed in one row, a number of teeth, the distance of which is regulated by the size of the car, and of which the upper extremities are recurved.

"'At the back of this car are two short pieces of wood like the poles of a litter. An ox is there harnessed with his head turned towards the car, sufficiently broken in to obey the driver.

Method of reaping.

"'As soon as the latter directs the carriage amongst the corn, the ears become entangled between the teeth, and are collected in the receptacle, being separated from the straw, which remains upon the field. The driver who follows the ox regulates the degree of elevation of the machine according to the height of the wheat. In this manner, in a few hours, by going and returning a few times, the harvest is soon finished. This method cannot be used but in level countries, and where the straw is not wanted.'"

The explanation of the drawing I have made, according to

summo reddant spatia largiora. Ab ejus fronte carpenti brevior est altitudo tabularum. Ibi denticuli plurimi, ac rari ad spicarum mensuram, constituuntur in ordinem, ad superiorem partem recurvi. A tergo verò ejusdem vehiculi duo brevissimi* temones figurantur velut amites basternarum. Ibi bos capite in vehiculum verso, jugo aptatur et vinculis, mansuetus sanè, qui non modum compulsoris excedat. Hic ubi vehiculum per messes cæpit impellere, omnis spica in carpentum denticulis comprehensa cumulatur, abruptis ac relictis paleis, altitudinem vel humilitatem plerumque bubulco moderante, qui sequitur, et ita per paucos itus ac reditus, brevi horarum s'patio tota messis impletur. Hoc campestribus locis, vel æqualibus utile est, et iis quibus necessaria palea non habetur. *Lib.* 7,° *tit.* 2 *pag.* 347, *edit Lugduni, anno* 1535. *Pag.* 146, *edit Biponti, anno* 1687.

* The shafts cannot be so short as the author says, *brevissimi temon s.* They ought to exceed the length of the ox, in order that the driver, who is behind, may move them without trouble or fatigue to the animal.

to

Theory of the Tides

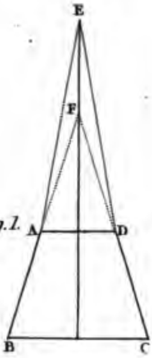


Fig. 1.

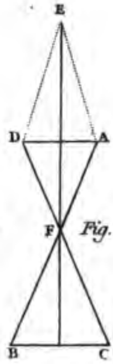


Fig. 2.

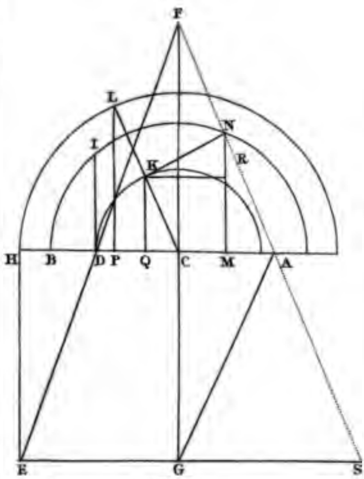


Fig. 3.

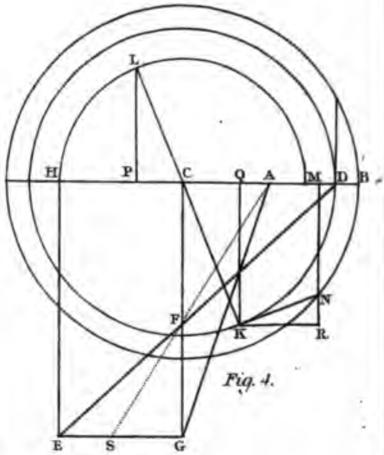
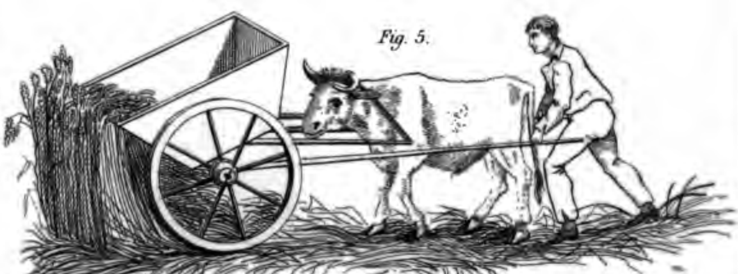


Fig. 4.

Fig. 5.



this description, will give a perfect notion of this machine, in which I have only made some very slight alterations which I thought necessary.

Description of the Design, Fig. 5. Plate IV.

Fig. 1. A. B. a box in the shape of a kneading trough, mounted obliquely on the frame of the carriage, so that the driver may incline it more or less by means of the assistance of the two long poles CD, and EF.—G. H. shafts of the carriage to which the ox is harnessed by the straps; I. M. is the board in the front of the box, which is not so high as the others, and upon the edge of which are a row of iron points curved inward, and sufficiently close, so that when the straw is engaged between them, and the machine advances forward, the ears cannot clear themselves, but are separated from the straw by the effort the ox makes to advance. Reference to the engraving.

It appears, that in those times this machine was used, the straw was left on the ground in order to enrich it, and was buried in the earth by repeated ploughing. If this machine were again brought into use, after the corn was removed, might not the straw be removed in the same manner as grass is done? If this were the only disadvantage that presented itself*, it might soon be obviated; but I think much corn would be lost, and in this point of view, this machine, so far from being economical, would be prejudicial in agriculture. It requires to be considerably improved: but this is not my present object: I have proposed merely to give a description of it. Conclusion.

* It is probable this machine was invented to carry off the grain in haste, on the fear of invasion from an enemy.

V.

METEOROLOGICAL JOURNAL.

1813.	Wind	Barometer. Max.	Min.	Med.	Thermometer. Max.	Min.	Med.	Evap.	Rain.	
4th Mo.										
April 24	N E	30·14	29·95	30·045	—	35	—	—		
25	N E	29·95	29·71	29·830	53	43	48·0	—	·51	
26	S E	29·71	29·51	29·610	—	39	—	—		
27	N W	29·51	29·36	29·435	48	45	46·5	—	·52	
28	N	29·61	29·36	29·485	50	41	45·5	·15	4	
29	N E	29·65	29·61	29·630	49	39	44·0	—	·21	
30	E	29·65	29·63	29·640	—	—	—	—	—	●
5th Mo.										
May. 1	N E	29·72	29·63	29·675	59	45	52·0	—	·25	
2	Var.	29·79	29·72	29·755	61	46	53·5	—		
3	E	29·79	29·73	29·760	66	50	58·0	·17	·17	
4	Var.	29·86	29·73	29·795	65	51	58·0	—	·24	
5	N W	29·91	29·87	29·890	64	49	56·5	—	—	
6	N E	29·87	29·69	29·780	68	50	59·0	—	·16	
7	S E	29·70	29·69	29·695	69	45	57·0	—	8	☽
8	S E	29·70	29·57	29·635	68	49	58·5	·27	—	
9	N	29·73	29·57	29·650	72	52	62·0	—	—	
10	Var.	29·90	29·73	29·815	69	47	58·0	—	8	
11	Var.	29·73	29·60	29·665	67	53	60·0	—	5	
12	S W	29·64	29·60	29·620	74	49	61·3	—	3	
13	S W	29·64	29·46	29·550	72	51	61·5	·35	—	
14	Var.	29·47	29·39	29·430	68	52	60·0	—	9	
15	S W	29·70	29·57	29·635	65	47	56·0	—	—	○
16	S W	29·72	29·41	29·565	65	49	57·0	—	·51	
17	N W	29·82	29·68	29·750	60	47	53·5	—	—	
18	Var.	29·80	29·78	29·790	60	49	54·5	·45	·25	
19	S W	29·84	29·56	29·700	69	50	59·5	—	—	
20	W	29·59	29·57	29·580	61	42	51·5	—	—	
21	W	29·64	29·59	29·615	59	39	49·0	—	—	
22	W	29·69	29·59	29·655	58	30	44·0	·28	·53	
		30·14	29·36	29·678	74	30	54·79	1·67	3·72	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Fourth Month. 24. Heavy *cumulostratus* clouds through the day. 25. Rain, nearly the whole day. 26. The *maximum* of temperature, at 9 a. m. cloudy : clear at evening, with *cirri*. 27. A wet day. 28. Wet morning : cloudy. 29. 30. Cloudy : much wind.

Fifth Month. 1. The *maximum* of temperature, at 9 a. m. wet. 2. Cloudy a. m. In the afternoon the sky cleared pretty suddenly, save that some dense *cumulus* clouds remained in the N. E. to the summit of one of which a *cirrostratus* was observed for a considerable time adhering, which was at length incorporated with the larger cloud. The moon appeared with a pale, golden crescent, the remainder of the disk being pretty conspicuous. 3. Dense *cumulus* clouds to the S. with *cirrus* and *cirrocumulus* intermixed (as before thunder.) A shower of large drops about sunset. 4. Overcast sky, a. m. About 6 p. m. (after some previous dripping) a thunder storm, the weight of which fell to the E. of us. A most brilliant rainbow, together with a complementary one, was exhibited for about 40 min. The space included *within* the proper bow was very perceptibly *lighter*, and that *without* it, extending to the complementary arch, as much *darker* than the rest of the cloud. A nightingale sang with spirit in the midst of the shower. 6. a. m. much dew : p. m. a large *nimbus* in the N. *Cirrostratus* in the E. and *cirrus* above, stretching from E. to W. The large cloud moved away by W. into the S. A thunder storm ensued in that direction, though nearly out of hearing, and lasted till midnight ; after which we had a sudden heavy shower. 7. a. m. cloudy : p. m. (after a shower) clearer, but with indications of more rain. 8. An appearance of much electrical action in the clouds far to the S. and S. W. 9. A few drops of rain a. m. various modifications of cloud appeared this day. 10. *Nimbi* ; dripping afternoon : rainbow : fine evening. 13. Cloudy, windy. 14. Much wind. 15. The same : calm night. 16. 17. 18. Much wind : showers.

RESULTS.

Winds variable.

Barometer : greatest height, 30·14 in. ; least 29·36 in.

Mean of the period 29·678 inches.

Thermometer : greatest height 74° ; least 30° ;

Mean of the period, 54·79°.

Evaporation (the guage being now placed on the ground) 1·67 in.

Rain 3·72 in.

A guage placed against the north wall of the house, at about seven feet elevation, gave, for the evaporation in the same time, only 1·16 inches.

L. HOWARD.

TOTTENHAM,
Sixth Month, 9, 1813.

VI.

Notice of a Prize offered by the Medical Society of Edinburgh, for the best Essay on the Question, "Whether Azote be absorbed during Respiration."

Edinburgh, 7th May, 1813.

SIR,

YOU will confer an obligation on the Medical Society of Edinburgh, by inserting in your valuable publication the following paragraph:

The Royal Medical Society of Edinburgh propose, as the subject of the prize essay for the year 1815, the following question:

"Is azote gas absorbed in the lungs during respiration? If it is not, whence do herbivorous animals derive their azote?"

A set of books, or a medal of five guineas value, shall be given to the author of the best dissertations on an experimental investigation of the subject proposed, for which all the members honorary, extraordinary, and ordinary, are alone invited as candidates.

The dissertations are to be written in English, French, or Latin, and to be delivered to the secretary, on or before the 1st of December of the succeeding year, to that in which the subjects are proposed. And the adjudication of the prize shall take place in the last week of February following.

To each dissertation, shall be prefixed a motto, and this motto is to be written on the outside of a sealed packet, containing the name and address of the author. No dissertation shall be received with the author's name affixed, and all dissertations, except the successful one, will be returned, if desired, with the sealed packet unopened.

I am, Sir,

Your most obedient Servant,

N. BAIN, Secretary.

To William Nicholson, Esq.
London.

Letter

VII.

Letter from a Correspondent respecting an Error in Mr. Woodhouse's Elementary Treatise on Astronomy.

To Mr. Nicholson.

March 24th, 1813.

SIR,

THE Elementary Treatise on Astronomy published last year by Mr. Woodhouse, of Caius College, in the University of Cambridge, is, I make no doubt, in the hands of most of your astronomical readers; permit me to offer, through the medium of your Journal, a few observations on the method the learned author has there adopted for adjusting a siderial clock to $0^h. 0'. 0''$.

Having determined, by means of the differential series, $y = a + dx + d^2 x \frac{(x-1)}{2} +$, &c. the time when the sun entered the equator, which in the example selected, is March 20th, $18^h. 11'. 23''$. for meridian of Cambridge; he concludes, that at that time the first point of aries was on the meridian. Now, Sir, if the first point of aries was on the meridian, it is manifest that the sun, which is on the 1st point of aries, is also on the meridian, that is to say, at $18^h. 11'. 23''$. mean solar time, a phenomenon which I believe no astronomer has ever yet witnessed. The error into which the learned and ingenious author has fallen, might possibly perplex some of the younger persons for whom the treatise is destined, and therefore I have chosen to point it out through the medium of your valuable publication.

I am, Sir,

Your constant reader,

D. T.

VIII.

Observations on the ebbing and flowing Well at Giggleswick, in the West Riding of Yorkshire, with a Theory of reciprocating Fountains. By Mr. JOHN GOUGH*.

Defect of early theories.

WHEN a theory happens to be formed from the comparison of a few facts only, future observations frequently perplex it with difficulties which are not easily surmounted. It is not necessary to seek for examples to corroborate the preceding assertion; for, in all probability, most philosophers will be able to establish the truth of it, by incidents which are preserved in the private histories of their own speculations. In my opinion, however, the writers on hydraulics furnish a striking instance of the fact in the machinery, which they commonly employ for the purpose of explaining the causes of reciprocating fountains, or of ebbing and flowing wells, as they are called in vulgar language.

Ebbing and flowing springs seldom occur in nature.

Springs of this description may be reckoned amongst the rare productions of nature; the infrequency of which leads me to conclude, that but few thinking men have had an opportunity of observing a number of them with attention, and of comparing their operations; for it is certain, that by far the greatest part of the world knows nothing of the subject, except by report. This want of ocular information, in all probability, has obliged speculative writers to rest content with the few facts which are to be found in books; and I am only acquainted with the following narratives which can be said to throw any light on the curious properties of reciprocating fountains. The first that I shall mention, came from the pen of the younger Pliny, who flourished as a statesman and a man of letters, in the time of Trajan. The account may be found in the concluding letter of the fourth book of his epistles; and the following is an attempt to give it in my own language, as I have no translation of the work in my possession.

Two narratives which throw light on the subject.

PLINY *to* LICINIUS.

Spring described by the younger Pliny.

"I am going to present you with a description of a natural curiosity in the neighbourhood of my country house, in hopes that it will prove an interesting speculation to a person of your extra-

* Manchester Soc. Mem. II. New Series, 354.

extraordinary attainments. A spring rises on the side of a mountain, and runs along a rocky channel into an artificial basin placed in a summer house, where it is for some time detained, and then falls into the Larian Lake. This fountain possesses a surprising property, for it flows and ebbs thrice a day, observing a stated law of increase and decrease. This singular circumstance may be observed with ease, and is calculated to amuse the spectator. You may sit in the apartment, make a slight repast, and drink of the water of the fountain, which is deliciously cool. In the mean time, the reciprocating motion of the spring proceeds equally, and in a manner which is easily ascertained by placing a ring, or any other small object upon a dry part of the basin. The water will rise gradually to the mark, and afterwards cover it. The fountain will at length subside, so as to leave the object dry, and will be afterwards seen to retire slowly. If you prolong your stay, these alternate motions will be repeated two or three times. Is this singular appearance occasioned by air acting upon the outlet of the fountain, so as to obstruct the current when it enters by the mouth of this channel, and after its escape, to allow the water to issue more freely? We know this to be the case with bottles, and all kind of vessels which have narrow necks—for when they are placed in a position proper for discharging their contents, the resistanceofthe air makes them guggle, and the liquor issues from them in an interrupted stream. Or does this fountain partake of the nature of the ocean? Is its current retarded at one time, and accelerated at another, by the causes which give rise to the flux and reflux of the sea? Rivers, we know, are driven back, when they fall into the sea against the wind and tide. May not some cause, in like manner, periodically obstruct the discharge of this fountain? Or, are we to suppose, that the subterranean veins of the fountain have a certain capacity; and that while they are recruiting their exhausted stores, the stream is small and languid; but becomes stronger and more abundant, when these reservoirs are replenished? Or is there a secret and unknown contrivance of a stop acting on the principle of a balance; which accelerates the efflux of the fountain while it empties itself, and diminishes the current while it is filling."

It reciprocated slowly by a rise and fall.

Conjectures, that it may be caused by obstructions of air;

or by a tide;

Or by reservoirs filling and emptying, &c.

The two last suppositions are obscurely expressed in the original;

Hypothesis of

a rocking stone forming a valve.

original; the latter of them, however, seems to have suggested the hypothesis of a rocking stone; which, acting on the principle of a valve, alternately opens and shuts the outlet of the spring; and my translation is made to favour this conjecture. The elder Pliny also mentions the same fountain, and ascribes to it a very remarkable and unaccountable difference; for he asserts, that it ebbs and flows regularly in the space of an hour. HIST. NAT. *Lib. II. Cap. ciii.* We are surprised to find the uncle and nephew, both intelligent and observing men, vary so widely in the statement of an obvious fact. Their disagreement, however, does not contradict the regularity of the spring's operations, which is a consideration of importance in the natural history of reciprocating fountains. As for the question of accuracy, it has been decided in the uncle's favour by Catanaeus, the learned commentator on the epistles of the nephew; who says, the fountain continued to reciprocate in his time, that the neighbours called it Pliny's well, and that it answered to the description given of it by the elder writer of that name. After all, future observations may prove both these authors to be in the right. Perhaps it will be found, that wet weather accelerates the reciprocations of the spring, by increasing its discharges; while a dry season diminishes the efflux of water, and makes the fountain more dilatory in its operations. The preceding conjecture is countenanced by the reciprocating spring at Giggleswick; for it ebbs and flows most frequently after copious rains; but the depth of the well shews the greatest variations when the efflux is but small.

Variation in the elder Pliny's description.

Wet weather appears to affect these springs.

Another reciprocating spring in Dodona.

The elder Pliny also takes notice of another reciprocrating spring, and gives the following short character of it with his usual brevity. "The fountain of Jupiter, in Dodona, extinguishes lighted tapers like any other cold water; but if a taper be first extinguished, and then brought to the surface of the well, it takes fire again. This fountain is called ΑΝΑΠΑΥΜΕΝΟΣ that is, the *Loiterer*, because it is empty at noon; but beginning to increase after mid-day, it overflows in the middle of the night, and then subsides again gradually." HIST. NAT. *lib.* II. *cap.* ciii.

Strong fountain in Westphalia.

A third extraordinary fountain of this kind is mentioned by various modern authors. It is said to be in Paderborn, a district of Westphalia, and to go by the name of Bolder-born, or the boisterous brook. This is an appellation which it deserves; for after flowing twenty-four hours, it ceases for six hours; at the end of which period it returns with a great noise, and force sufficient to turn three mills, situated near its visible source. The operations of this fountain are differently described in the Philosophical Transactions, where it is said to lose itself twice in twenty-four hours; coming always after six hours back again. Lowthorp's Abridgment Vol. II. p. 306. ment, Vol. II. Page 305.

The prevailing opinion, respecting the nature of reciprocating fountains appears to be derived from the three preceding instances; at least I am not acquainted with any other topographical account, which can be said to favour the notion on rational, or even on probable principles. This theory may be found in many popular works on natural philosophy; and it is easily explained by the hydraulic machine called Tantalus's cup. This instrument consists of a vessel furnished with a siphon, which may be attached to it in different ways. To avoid the necessity of a diagram, we will suppose the bottom of the vessel to be perforated, and the longer leg of the siphon to pass through the hole, being firmly cemented in a position, which places the highest point of the bend within the vessel, and half an inch or an inch below the brim, and at the same time keeps the open or lower end of the shorter leg at a small distance from the cup's bottom. Water flows through a tube in an uniform stream into the cup; where it is collected for want of egress, and entering the siphon at the open end of the shorter leg, it rises gradually to the bend or highest point. The subsequent rise of the water in the cup, forces the column in the ascending leg of the siphon, to pass over into the descending or longer branch; upon which this instrument begins to act, not in the manner of a simple tube, but in its proper character. Now the draft of the siphon is made to exceed the opposite stream or supply of water; in consequence of which contrivance the cup is emptied again sooner or later; at this moment the action of the siphon is suspended, until the cup is replenished by the constant current. In this manner the water will be seen rising and falling alternately in the cup, which will be full and empty, or nearly so, by turns. Similar vicissitudes will also take place in the siphon; for it will run so long as its shorter leg

General theory of a siphon, like that in the cup of Tantalus.

Description of the apparatus so called.

leg is in the water, and then stop, until the highest point of the bend is again covered by the contents of the cup.

Application of the effect in the cup to explain the phenomena of variable springs.

The transition is easily made from Tantalus's cup to a fountain, which reciprocates periodically; for we have only to suppose a secret reservoir to be formed in the bowels of a mountain on the principles of this instrument, and the following appearances will take place in the visible well, which receives the water from the natural siphon. 1st. So soon as the surface of the pool in the subterranean reservoir, rises above the bend of the siphon, this canal will begin to act; and its discharge will be greater at that moment than at any other period; because the power of a siphon is greatest, when the distance, betwixt the bend and the surface of the water in the basin, is least. 2d. This abundant influx into the external well will make it rise; in consequence of which the efflux will continue to increase at the outlet, so long as the water continues to accumulate in the visible basin. 3d. Now the discharge from the outlet, which becomes more copious every moment, being contrary to the influx from the siphon, which grows gradually weaker, the surface of the well will cease to rise so soon as these opposite powers are equal in their effects; and the flow will be at the full in this instant. 4th. The well cannot remain stationary, for any length of time, at its highest elevation; because the vigor of the siphon being perpetually on the decline, all the water discharged by it will run off through the outlet, together with part of that, which had been previously accumulated in the visible fountain, during the time of the flow. 5th. Hence it is evident that the well will begin to subside, the moment it becomes stationary; after which, it will persevere in a retrograde motion, until the siphon shall have emptied the subterranean reservoir. 6th. If no veins of water discharge themselves into the visible basin, besides the siphon which runs periodically, the spring is called an INTERMITTING fountain. The Bolderborn is of this kind, for it remains dry while the secret reservoir is filling, and flows while the siphon is in action. 7th. But if the spring receives other supplies in addition to the intermitting current, it is called a RECIPROCATING fountain; because the stream that issues from the outlet of the visible basin is permanent, though it varies in quantity; on this account the well ebbs and flows alternately, but never runs itself dry. All the fountains, which will be mentioned in the sequel

Difference between intermitting and reciprocating springs.

sequel, are of this kind; and Pliny's well, near Coma, appears to possess the same character from his description of it. 8th The fluctuations of an ebbing and flowing well, which is fed by a siphon, will remain invariable, so long as the stream, that falls into the subterranean reservoir continues to be uniform. But these external and visible operations of the well, are so far under the influence of the current last mentioned, that they will evidently suffer a temporary suspension, so often as the influx into the concealed cistern, amounts to a certain quantity in a certain time; for the siphon is but a secondary agent in producing the phenomena of reciprocation, its business being to empty the subterranean basin, so often as it is replenished. Now the time of filling this magazine of water will be the shortest, when the influx into it is most abundant, and the contrary; consequently an increased discharge into the subterranean reservoir, will diminish the intervals of the siphon's inactivity, and prolong the periods of its action. It follows, from these premises, that when the influx becomes equal to the feeblest effort of the siphon, the quantity of water thrown into the concealed basin, will exactly counterbalance the quantity which is drawn off by the crooked canal: and the external *well* will assume the character of a common fountain under these circumstances.

The fluctuations of an ebbing and flowing well will be fewer the greater the supply &c.

I have now explained the principles, on which the common theory of reciprocating springs is founded; and the necessary consequences of the theory are stated in the eight preceding propositions. This has been done, to shew with what ease a natural apparatus on the construction of Tantalus's cup elucidates the appearances, which have been ascribed by writers to the fountains of Dodona, Coma, and Paderborn. The operations of these springs are happily illustrated by the instrument in question; on which account I do not hesitate to pronounce the theory to be a good one, so far as it relates to these fountains alone; provided they are faithfully described. The simplicity of the preceding explanation, and its coincidence with the narratives of the two Plinys, as well as the history of the inconstant brook in Westphalia, disposed me to admit the common theory, and to imagine it to be equally applicable to reciprocating fountains in general; until an instance occurred to my notice, which proved that, fluctuating fountains do not universally exhibit the periodical operations which are described by

The springs before mentioned are well explained by the siphons;

the

But the Giggleswick well does not agree with that theory.

the writers already quoted. I made a visit to Giggleswick Well, in the autumn of 1796; which taught me to value this once favourite theory not so highly, and in particular to dispute the universality of its application. The causes of these doubts will be easily perceived from the following description of the well and its operations.

Description of this last well.

This spring lies at the foot of Giggleswick Scar, which is a hill of limestone in the West Riding of Yorkshire. The water discharged by it, falls immediately into a stone trough; in the front of which are two holes near the bottom; these are the outlets of two streams, that flow constantly from the artificial cistern. An oblong notch is also cut in the same side of the trough; which extends from the brim of it, neatly to the level of the two holes already mentioned. This aperture is intended to shew the fluctuations of the well: for the water subsides in it when the stream issuing from the rock becomes languid; on the contrary the surface of the water rises again in the notch, so soon as the influx into the trough begins to be more copious. The reciprocations of the spring are easily observed by this contrivance; and they appear to be very irregular both in respect of duration and magnitude. For the interval of time betwixt any two succeeding flows, is sometimes greater, and at other times less, than a similar interval which the observer may happen to take for his standard of comparison. The rise of the water in the cistern, during the time of the well's flowing, is also equally uncertain: for it varies from one inch, to nine or ten inches, in the course of a few reciprocations. It is necessary to remark on the present occasion, that the spring discharges bubbles of air, more or less copiously into the trough; these appear in the greatest abundance at the commencement of a flow, and cease during the ebb, or at least issue from the rock very sparingly at that time. In fact, the appearance and disappearance of these bubbles, are circumstances equally inconstant with the rise and fall of the water.

It reciprocates very irregularly, and discharges air bubbles.

The irregularites exhibited by the ebbing and flowing well, during my short visit, diminished the respect which I ormerly had for the popular theory, more especially when considered as a general explanation of reciprocating springs. This change of opinion was suggested by the caprices of the well; which were too many and too singular to be ascribed to the uniform operation tions of a single siphon, as we have seen already; and the accidental combination of several siphons in one fountain, is a conjecture too improbable in itself to demand a serious discussion. My suspicions respecting the accuracy of the principle were not a little increased, by the following descriptions of two reciprocating fountains. Weeding Well, in Derbyshire, appears to be more fickle and uncertain in its reciprocations, than the well at Giggleswick. Dr. Plot describes this remarkable fountain, at page 41 of his history of Staffordshire, where he reports it to be very uncertain in its motions, ebbing and flowing sometimes thrice in an hour, and at other times not oftener than once in a month: he also quotes the following character of it, to the same import, from a Latin poem by Mr. Hobbs.

Weeding well is still more uncertain;

"Fons hic temporibus nec tollitur (ut Mare) certis:
"Æstibus his nullam præfigit Ephemeris horam."

The following account of a reciprocating fountain is extracted from an article in the second volume of Lowthorp's Abridgement, page 305; in which care has been taken to preserve the facts recorded by the author, Dr. W. Oliver, in language more concise than his own. "Lay Well, near Torbay, is about six feet long, five feet broad, and near six inches deep; it ebbs and flows very visibly; and many times in an hour. The reciprocations succeed each other more rapidly when the well is full, than they do when it is low. When once the fountain began to flow, it performed its flux and reflux in little more than a minute's time; but the Doctor observed it to stand sometimes two or three minutes at its lowest ebb; so that it ebbed and flowed about 16 times in an hour, by his watch. So soon as the water began to rise in the well, he saw a great number of bubbles ascend from the bottom: but when the water began to fall, the bubbling ceased immediately. The Doctor measured the distance betwixt the high and low water marks, not on a perpendicular line, but on a slope, and found it exceeded 5 inches.

and so likewise is Lay Well, near Torbay.

The three preceding instances of irregular reciprocation undoubtedly diminishes the importance of the popular theory, by proving that it is not of universal application; as it only explains the constitution of those fountains, which ebb and flow periodically. The Bolderborn of Westphalia, may be reasonably pronounced to be of this description; as for the fountain

Hence the popular theory is not universally applicable.

fountain of Jupiter in Dodona, we know too little of it to judge of its true character; and it is not improbable but future observations will add Pliny's Well to the class of irregular reciprocators.

It may be reasonably supposed, that since I have endeavoured to confine the established theory of reciprocation to one or two springs at most, a new explanation will be offered on my part, comprehending the phenomena of those wells, which ebb and flow according to no certain rule. Before I make this attempt, it will be proper to give a more circumstantial account of the appearances exhibited by the well at Giggleswick, than has hitherto been published. I neglected, when in the country, to preserve a correct register of its fluctuations, and committed no other observations to writing, except those which appear in a former part of this essay. This omission, however, has been fully supplied by Mr. John Swainston, of Kendal; to whom I formerly communicated my imperfect remarks on this well, requesting him at the same time to note down a series of its operations, at some convenient opportunity. This request was complied with by my friend; who has digested his observations in the following table, which merits the esteem of the naturalist, as being a faithful history of this singular fountain.

Account of the appearance at Giggleswick Well.

Table of the ebbing and flowing of Giggleswick well.

Observations made on Giggleswick Well, August 20th, 104, *from* 3 *to nearly* 6 *P. M.*

On first coming to the well it continued flowing near ten minutes, and then as in the Table.

No. of inches ebbed.	Time in Ebbing in minutes	Stationary at Ebb in minutes	No. of inches Flowed.	Time in flowing in minutes	Stationary at flow in minutes.
8½	4	7½	9	2	1½
1	1	—	½	—	1
—	—	—	½	—	—
1½	—	—	½	—	—
9½	4½	3	9½	4	2 ×
1	3	—	½	—	2
5½	3½	—	7	1	1
½	—	1	—	—	—
3	2	—	4	3	4 Basin 1 inch short of full.
6	3	—	7½	1½	1
6½	3	none	6	1	2½
6½	3½	—	7½	1½	1½ full
9	4½	2½	9	2	2
9½	4½	5½	9½	3½	1½ ×
½	½	3	—	—	—
1	—	3	—	—	Left it flowing over.
5	2½	none	6½	1½	

Mr.

Mr. Swainston has favoured me with the following explanatory remarks; which, perhaps, will throw some additional light on the history and properties of Giggleswick Well. In the two observations marked with crosses, the water flowed slowly for the first 3 or 4 inches, and then rose very quickly, until the cistern was full; the same appearance took place not unfrequently in the course of his remarks. Where the blanks are in the columns marked stationary at ebb, the water flowed again instantaneously; but there are some inaccuracies in this part of the table; for Mr. Swainston was interrupted more than once by travellers stopping to let their horses drink. The term stationary at ebb, signifies that the surface of the water in the cistern was stationary at its lowest elevation; at which time the discharge from the trough was commonly confined to the two holes near the bottom of it.

General remarks.

I have now stated all the facts in my possession, that relate to reciprocating springs. The fountains which have been described, are six in number, of these the inconstant brook in Westphalia, appears to require the agency of a siphon to account for its operations. The characters as ascribed to Pliny's Well, and the well in Dodona, are very ambiguous and unsatisfactory: but the operations of the three remaining springs, and more especially the register of Giggleswick Well, perplex the hypothesis of a siphon with insuperable difficulties; which a superficial inspection of the table will discover to the reader.

Additional, or new theory.

The theory, which I shall now propose for the explanation of irregular reciprocating springs, was suggested by an accidental observation; which occurred to Mr. Swainston, whom I have mentioned above. This gentleman, who is a manufacturer of Morocco-leather, has a contrivance in his works, for the purpose of filling a boiler of a particular construction with water. This apparatus consists of a tub, which is elevated considerably above the boiler. The water is conveyed from a pump along a trough into this vessel; from which it runs immediately into the upper extremity of an inverted siphon, which is cemented into a hole in the bottom. This compound tube consists of three branches or legs; the first descends perpendicularly beneath the tub, and is the longest of the three; the second ascends again and carries the water, which comes into it from the first, to a convenient height above

Mr. Swainston's apparatus in his manufactory, where water passed through a received siphon;

above the brim of the boiler; the third is a descending leg, which performs the office of a nozle, that is, it discharges the water from this crooked canal into the boiler. Mr. Swainston observed by accident, that when the workmen were filling the vessel last mentioned, the water reciprocated in the tub, the surface of it rising and falling alternately in a manner which he could not explain, by supposing some slight irregularity in the management of the pump. When the appearance was more carefully examined, he found a corresponding variation in the efflux at the nozle; for when the water was rising in the tub, the stream was perceptibly weaker at this outlet, than it was during the ebb or fall of the water in the vessel last-mentioned. He farther observed, that when the water in the boiler rose high enough to cover the end or nozle of the siphon, bubbles of air were seen ascending from this orifice, during the ebb in the tub, or at least during the former part of it; but that they did not appear during the flow, or whilst the water was accumulating in the tub. The fluctuations here described, were far from being regular, either in magnitude or duration; for the water rose much higher in the tub at one time, than it did at another; and the intervals betwixt flow and flow, or ebb and ebb, were very unequal. In fact the appearances seen in this vessel imitated the caprices and singularities of Giggleswick Well in a natural and surprising maner.

and was obstructed by intermissions by air.

The exact coincidence of the effects, produced by an artificial apparatus, and a noted reciprocating fountain, will naturally turn the attention of the curious to inquire into the cause of the irregular motions, which Mr. Swainton observed in his reservoir. The circumstance on which these fluctuations depended, is easily understood; for, seeing the inverted siphon discharged bubbles of air occasionally into the boiler, it is manifest that this subtle fluid entered the tube, mixed with the water, or in other words in the state of foam. Now it is well known, that the bubbles, constituting this frothy substance burst, and the air separates from the water, when the agitation ceases; by which the compound was produced. Such a separation would take place unavoidably in the siphon; because a current flowing in a tube moves on smoothly, or without interruption, which is the cause of agitation. The process

This fact agrees with those of the irregularly intermitting springs.

Particular explanation of Mr. Swainston's Apparatus, and its effect.

process here described, discovers the nature of the pheno-
n ... Mr. Swainston's vessel ; for the
a ... in the siphon, is col-
l ... t probably in a bend con-
r ... forms a bubble or mass,
... able obstruction in the
... the pipe. The water
... ong as its efflux is inter-
... e action of the stream in
... from place to place in its
... harged at the nozle. The
... store the stream to its full
... ll begin to subside in the
... so, until the surface arrives
... d collection of air happens
... We have now investigated
... servable in Mr. Swainton's
... om the obstruction of air
... canal ; the formation of
... t in a fortuitous or irregular
... ocation which results from
... to be equally uncertain and

... an ebbing and flowing vessel
... will be disposed to think,
... e of reciprocating fountains,
... is of these springs to the in-
... hich issues from a bottle. In
fact, only one ... is wanting to render his ex-
planation of the phenomenon complete; he has not informed his friend Licinius, how he supposes the air gets into the subterranean channel, which supplies his well with water. Perhaps this omission was the effect of design, rather than of negligence; for many philosophers in Pliny's time held the singular opinion, that the earth possesses the faculty of respiration like animals; in consequence of which it inhales and expires air through the crannies and caverns, which extend to its surface. Supposing Licinius to be of this way of thinking, Pliny had no reason to tell this ingenious and learned man, that he imagined the outlet of the fountain had a communi-
cation

Pliny discovered this theory by comparing the effects to the discharge of a bottle; but he did not account for the air.

Pages 161 - 169 were missing from vol. at time of microfilming.

cation under ground, with one of these spiracles of the globe. Be this as it may, the notion is too absurd to be mentioned in the present improved state of Natural Philosophy, in any other light than as a curious document of the puerile conceits with which the philosophers of ancient times amused their hearers. In the foregoing attempt to complete the theory, I have had recourse to a well known phenomenon; water is beaten into foam by being agitated; which was the case by Mr. Swainston's vessel, because a strong current fell into it from the pump. There is, however, one objection still remaining, which deserves to be considered: the levity of foam, compared with the superior weight of water, may lead some persons to suspect, that this light substance will not mix with water, but will float on the surface of the reservoir, in which it is formed. Supposing this suspicion to be well-founded for the sake of argument, we must allow the foregoing theory of reciprocating vessels to be defective in a very essential point; because if foam cannot sink, the air that proceeds from it, cannot find its way into the tubes or siphons, which convey the water from such vessels. Being unwilling to leave this objection unanswered, I resolved to put the truth of this principle to the test of direct experiment; which was done in the following simple manner: A small bell glass, being first filled with water, was inverted in six quarts of the same fluid, contained in a small tub. Things being thus prepared, the contents of the open vessel were agitated briskly; and the air which entered the water, found its way into the inverted glass, the upper part of which it occupied. The water of the tub was agitated by the motion of a whisk, or a bundle of slender twigs; it was some times taken up in a pitcher, and returned into the vessel quickly, from the height of a foot or more; both methods proved successful, but the former appeared to introduce air into the glass with more expedition than the latter did; the difference here mentioned, may however depend entirely upon management and accidental circumstances. The experiment which I have now related, shews the foregoing objection to be of no moment; consequently the present theory of irregular reciprocation may be pronounced to stand upon a safe foundation, and unexceptionable principles.

Experiments to shew that divided air in foam, &c. will descend.

The

The observations which have been made on Mr. Swainston's accidental discovery, render an elaborate inquiry into the constitution of Giggleswick well unnecessary. Nature may be easily supposed to have produced an apparatus in the side of the hill, possessing the mechanical properties of the reciprocating tub, and all the phenomena will follow, which are so remarkable in this fountain. Let us imagine a reservoir to be concealed from view under the rocks, into which the stream of a subterranean brook falls, and beats part of its contents into foam by agitation. Let this cavity be connected with the external or visible basin; by a narrow serpentine chink concealed in the interposing strata; and the reader must perceive, without farther explanation, that this conduit will perform the part of the inverted siphon already described, and exhibit the operations, as well as the irregularities of the fountain in question. The same internal structure may be supposed to exist in Lay Well, near Torbay; but something is required, in addition to this simple apparatus, to account for the casual reciprocation of Weeding Well, in Derbyshire. It is not a difficult task to accommodate the theory to the description of this spring; but when we consider how imperfect such descriptions are commonly found to be, it appears more adviseable to pass over this fountain in silence; until some accurate observer shall present the public with a correct and minute history of its operations.

Particular explanation of the effects in the Giggleswick well from the last stated theory.

All parties allow, that reciprocating fountains flow from pools of water concealed under ground; on which account it will not be very foreign to the topic of the present essay, if I conclude it with a few remarks on the structure and formation of caverns. I have visited many caves in this part of England, all of which are situated in the strata of calcareous hills. They also appear to have been once filled with an argillaceous stone, of a less durable nature than the surrounding limestone. This supposition is corroborated by the following fact: masses of clay, mixed with gravel, are found scattered up and down these hollows; and as they are lodged in chinks from which they cannot be easily removed by water, I suppose them to be the remains of extensive beds, which formerly occupied these recesses in the calcareous strata. This argillaceous matter, which choaked up the natural vaults of our limestone hills in early ages, has been gradually worn away by a simple but powerful agent.

Remarks on the structure of caverns.

The infiltration of rains through calcareous hills has washed out loose argillaceous matters,

agent. The rains which have fallen from the remotest times, constantly find their way through the chinks of the limestone; thus subterranean brooks were formed, which attacked the soft argillaceous matter situated under the harder covering of limestone. This perishable substance was first softened by the water, and afterwards broken down by the currents, which washed away the clay and gravel. In consequence of this alteration, the incumbent rocks of limestone were left to rely on themselves. Such, therefore, fell down as were not supported by mutual pressure, while the rest still remain suspended in the roof and sides of the caverns, being locked together like the stones of an arch. The agents which were formerly employed in the excavation of those subterranean chambers, remain, in many instances, to the present day; for almost every cavern is the place of union to a number of secret brooks, which enter it in different directions, some of them being perennial—but others depend on the weather. The impetuosity of these currents is very apparent in some caverns, which are filled with water in wet seasons—for the bottoms of them are covered with large masses of stone, the edges and angles of which are worn away, like those of a pebble that has been rolled in the channel of a rapid river.

and left the limestone roof to support itself, or fall; as, in fact, is seen to have happened.

Caverns mostly have streams in them.

Why caves are most commonly in calcareous strata.

I have already remarked, that the caves of the North of England are commonly found in calcareous strata. This circumstance may be traced to natural causes—for the rain water descends with great ease through the vertical fissures of these rocks, which generally rest upon a base of grey schist, and in some places on a soft argillaceous substance of a laminated texture. This base is not uniformly flat, for it swells occasionally into lumps or hillocks, some of which appear above the surrounding limestone. Such of these hillocks as were originally situated under one, or a number of subterranean brooks formed in the calcareous strata, have been washed away long ago; and the caverns which remain at present, shew the extent and form of these demolished eminences. The recesses thus produced, frequently contain pools of water; and if the presence of a grotto be necessary to a reciprocating fountain, perhaps few places are more likely to produce one, than the neighbourhood of Giggleswick. For the country abounds with caves, and also with subterranean brooks; one of which is heard very distinctly

distinctly through the rocks which cover it, at a place where it sounds like a stream falling into an extensive chamber.

IX.

Observations of a second Comet, with Remarks on its Construction. By William Herschel, *LL. D. F. R. S.*

As we have lately had two comets to observe at the same time, I have called that of which the following observations are given, the second. Its appearance has been so totally different from that of the first, that every particular relating to its construction becomes valuable; and, notwithstanding the unfavourable state of the weather at this time of the year, I have been sufficiently successful to obtain a few good views of the phenomena which this comet has afforded. The present comet differed from the former,

A short detail of the observations, in the order of their relation to the different cometic appearances, is as follows:

The Body of the Comet.

January 1, 1812.—I viewed the second comet with several of my telescopes, and found it to have a considerable nucleus surrounded with very faint chevelure. having a large nucleus and chevelure.

Jan. 2.—The comet had a large round nucleus within its faint nebulosity. Not seeing it very well defined, and of so large a diameter, I doubted whether it could be the body of the comet; but although it might be called very large when supposed to be of a planetary construction, it was much too small for the condensed light of a head; its diameter, by estimation, not exceeding five or six seconds.

By way of comparing the two comets together, I viewed them alternately. The first within a nebulosity which, in the form of a brilliant head, was of great extent, had nothing resembling a nucleus: the light of this head was very gradually much brighter up to the very middle; its small planetary body being invisible. The second comet, on the contrary, although surrounded by a faint chevelure, seemed to be all nucleus; for the abrupt transition from the central light to that of the chevelure, The former has a brilliant nebulous head, brightest in the middle.

velure, would not admit of the idea of a gradual condensation of nebulosity, such as I saw in the head of the first comet; but plainly pointed out that the nucleus and its chevelure were two distinct objects.

Examinations with different powers.

Jan. 8. The comet had a pretty well defined nucleus with very faint chevelure. When magnified 170 times the nucleus, though less bright, was rather better defined.

Jan. 18. Within a very faint chevelure I saw the nucleus as before.

Jan. 20. The air being uncommonly clear, I saw the body of the comet well defined; and as the moon was already so far advanced in its orbit as to render future opportunities of viewing the comet very improbable, I ascertained the magnitude of its body, with a very distinct 10 feet reflector, by the following three observations:

First with a low power, which gave a bright image of the nucleus, I kept my attention fixed upon its apparent size; then looking away from the telescope, I mentally reviewed the impression its appearance had made on the imagination, in order to see whether it was a faithful picture of the object; and by looking again into the telescope I was satisfied of the similitude.

In the next place I used a deeper magnifier, and alternately viewed and remembered the appearance of the nucleus. It was fainter with this power.

The third observation was made in the same manner with a magnifier of 170. This showed the nucleus of a larger diameter, but much less bright, and not so well defined.

The nucleus subtended 5¼ seconds.

The next morning, having recourse to my usual experiment with a set of globules, by viewing them at a given distance with the same telescope and eye-glasses, I found that one of them, on which I fixed, gave me, as nearly as could be estimated, the same magnitude with the first eye-glass, and was proportionally magnified by the second and third, with only this difference, that the highest power showed the globule with more distinctness than it did the nucleus; and by trigonometry the angle under which I saw the globule was found to be 5″,2744*.

* I prefer this method of ascertaining the small diameter of a

It will be necessary to mention that in the calculations belonging to this comet, I have used the elements of Mr. Gaus, with a small correction of the longitude of the perihelion, which I found would answer the end of giving the observed place with sufficient accuracy from the 1st of January to the 20th. These calculations may however be repeated, if hereafter we should obtain elements improved by additional observations, made with fixed instruments; but the result, I may venture to say, will not be materially different.

The distance of the comet from the earth, the 20th of January, when its apparent diameter was determined, was 1,0867, the mean distance of the earth from the sun being 1; whence we deduce a very remarkable consequence, which is, that the real diameter of its nucleus cannot be less than 2637 miles. And its diameter 2637 miles.

The Chevelure of the Comet.

Instead of that bright appearance, which in the first comet has been considered as the head, there was about the nucleus of the second a faint whitish scattered light, which may be called its chevelure. Examination of the tail.

Jan. 1. Examining the chevelure of the comet with a 10 feet reflector, I found that it surrounded the nucleus, not in the form of a head consisting of gradually much condensed nebulosity, but had the appearance of a faint haziness, which although of some extent, was not much brighter near the nucleus than at a distance from it.

Jan. 2. I viewed the two comets alternately. The first could only be distinguished from a bright globular nebula by the scattered light of its tail, which was still 2° 20′ long. The second comet, on the contrary, had nothing in its appearance resembling such a nebula: it consisted merely of a nucleus, surrounded by a very faint chevelure; and had it not been for an extremely faint light in a direction opposite to the sun, it would hardly have been entitled to the name of a comet;

faint object to measuring it with a micrometer, which requires light to show the wires, and a high magnifying power to give an image sufficiently large for mensuration; neither of which conditions the present comet would admit.

having

having rather the appearance of a planet seen through an atmosphere full of haziness.

Jan. 8. The chevelure consisted of so faint a light that, when magnified only 170 times, it was nearly lost.

Jan. 18. The chevelure was extremely faint and of very little extent.

Jan. 20. The light of the moon, which was up, would not admit of further accurate observations on the chevelure.

The Tail of the Comet.

It measured 9′ 40″ and was about 659 thousand miles long.

Jan. 1. With a low magnifying power, I saw in the 10 feet reflector an extremely faint scattered light, in opposition to the sun, forming the tail of the comet. It reached from the centre of the double eye-glass half way toward the circumference.

Jan. 8. The narrow, very faint scattered light beyond the chevelure remains extended in the direction opposite the sun.

Jan. 18. I estimated the length of the tail by the proportion it bore to the diameter of the field of the eye-glass, which takes in 38′ 39″, and found that it filled about one quarter of it, which gives 9′ 40″.

Jan. 20. On account of moonlight the tail was no longer visible.

From the angle which it subtended in the last observation, it will be found that its length must have been about 659 thousand miles.

Remarks on the Construction of the Comet.

The body of this comet approached to planetary density.

The method I have taken in my last paper of comparing together the phenomena of different comets appears to me most likely to throw some light upon a subject which still remains involved in great obscurity. When the comet of which the observations have been given in this paper is compared with the preceding one, it will be found to be extremely different. Its physical construction appears, indeed, to approach nearly to a planetary condition. In its magnitude it bears a considerable proportion to the size of the planets; the diameter

meter of its nucleus being very nearly one-third of that of the earth.

The light by which we see it is probably also planetary; that is to say, reflected from the sun. For were it of a phosphoric, self-luminous nature, we could hardly account for its little density: for instance, the very small body of the first comet, at the distance of 114 millions of miles from the earth, bore a magnifying power of 600, and was even seen better with this than with a lower one;* whereas the second, notwithstanding its large size, and being only at the distance of 103 millions, had not light enough to bear conveniently to be magnified 107 times; but if we admit this nucleus to be opaque, like the bodies of the planets, and of a nature not to reflect much light, then its distance from the sun, which the 20th of January was above 174 millions of miles, will explain the cause of its feeble illumination.

Its light appeared to be reflected or planetary.

That the nucleus of this comet was surrounded by an atmosphere appears from its chevelure, which, though faint, was of considerable extent: and the elasticity of this atmosphere may be inferred from the spherical figure of the chevelure, proved by its roundness and equal decrease of light at equal distances from the centre.

Its atmosphere was considerable,

The transparency of the atmosphere is partly ascertained from our seeing the nucleus through it, but may also be inferred by analogy from an observation of the first comet. It will be remembered that an atmosphere of great transparency, which had been seen for a long time, was lost when the comet receded from the sun, by the subsidence of some nebulous matter not sufficiently rarified to enter the regions of the tail.† Now as the existence of this atmosphere, when it was no longer visible, might have been doubted, the luminous matter suspended in it, which had already 20 days obstructed our view of it, happened fortunately to be once more elevated the 9th of December, and thereby enabled us, from its transparency and capacity of sustaining luminous vapours, to ascertain the continuance of its existence. By analogy, therefore, we may surmise that the faint chevelure of the second comet

and transparent.

* See Observations of the First Comet.

† See Observations of the First Comet.

consists

consists also of the condensation of some remaining phosphoric matter, suspended in the lower regions, of an elastic, transparent fluid, extending probably far beyond the chevelure without our being able to perceive it.

The faintness of the tail did not arise from its great perihelion distance.

We might ascribe the little extent and extreme faintness of the tail to the great perihelion distance of the comet, if it had not already been proved, by the comparative view which in my last paper has been taken of the two comets of 1807 and 1811, that the effect of the solar agency depends entirely upon the state of the nebulous matter, which the comet in its approach exposes to the action of the sun. Our last comet therefore had probably but little *unperihelioned* matter in its atmosphere.

Other proofs of the high consolidation of this comet.

The high consolidation of the matter contained in the second comet is also much supported by the different appearance of the two comets in the observation of the 2d of January. In order to judge of them properly, we must consider their situation with regard to the sun and the earth; the first comet was 192 millions of miles from the sun; the second only 16.: the first was at the same time 262 millions from the earth: the second only 83; but, notwithstanding the great disadvantage of being 28 millions of miles farther from the sun, and about 179 millions farther from the earth, the first comet had the luminous appearance of a brilliant head accompanied by a tail 45 millions of miles in length; whereas the second comet, so advantageously situated, had only a very faint chevelure about its large but faint nucleus, with a still fainter tail, whose length has been shown not much to exceed half a million.

Results.

If then the effect of the action of the sun on the comets at the time of their perihelion passage is more or less conspicuous, according to the quantity of unperihelioned nebulous matter they contain, we may by observation of cometic phenomena arrange these celestial bodies into a certain order of consolidation, from which, in the end, a considerable insight into their nature and destination may be obtained. The three last observed comets, for instance, will give us already the following results.

This comet would have been not more affected than

The comet of which this paper contains observations, is of such a construction that it was but little more affected by a perihelion passage than a planet would have been. This may be be ascribed to its very advanced state of consolidation, and to its having but a small share of phosphoric or nebulous matter in its construction. a planet in a perihelion pas.

That of the year 1807 was more affected, and although considerably condensed, showed clearly that it conveyed a great quantity of nebulosity to the perihelion passage. That of 1807 was more nebulous.

The comet of last year contained with little solidity a most abundant portion of nebulous matter, on which, in its approach to the perihelion, the action of the sun produced those beautiful phenomena, which have so favourably afforded an opportunity for critical observations. And the preceding comet was still more so.

X.

Remarks on the Summer Birds of Passage, and on Migration in general. By Mr. John Gough.

PERHAPS no phenomenon in the history of animated nature has engaged the attention of men of observation, in all ages and countries, so generally as the regular appearance of those birds which visit the northern climates in spring, and disappear as regularly at the approach of winter. But though many facts have been collected, relating to the manners of these singular birds, by the industry of naturalists, their history still remains involved in much obscurity, and perplexed with difficulties; many of which, in my opinion, arise from a negligent or an injudicious arrangement of the facts already ascertained. Philosophers have been induced by this oversight, to take partial views of the subject; and to entertain very discordant notions respecting the winter retreat of the birds in question. All parties, however, are unanimous in concluding, that the regularity of their visits in spring is intimately connected with the apparent motion of the sun betwixt the tropics, whose northern declination is increasing at the time of their appearance, and consequently the temperature of the northern hemisphere is also advancing towards the heat of summer in every latitude. The philosophers, who have undertaken to discuss this curious question in natural history, agree, then, in ascribing the alternate appearance and disappearance of the swallow tribe, the cuckoo, the wryneck, and a majority

The migration of birds is not yet well understood.

It depends on the seasons.

jority of the British warblers, to the vicissitudes of temperature which are annually experienced in this country, in common with all other places at a distance from the equator. But their unanimity ends here; and, at this point, they split into two parties, who view the subject in very different lights. I intend to state the opinions of each in succession, beginning with those philosophers who appear to me to have the less degree of probability in their favour; or, to speak more properly, whose notions cannot be defended on their own principles, when these are carefully examined.

Opinions of philosophers. Pliny, that they retire to caverns.

Pliny is the oldest naturalist that I recollect, who maintains, that the swallow tribe, and many other birds, with whose winter quarters he was unacquainted, retire to caverns at the end of autumn, where they lie in a torpid state until the return of spring. Many moderns have embraced this idea; and they conclude from a familiar analogy, that the sun, after making certain advances towards the north, recalls these sleepers from a lethargic state, to active existence, in the same manner, that he breaks the winter slumbers of the bat, the field-mouse, and the edge-hog; as well as of various reptiles, and insects inhabiting the temperate and frigid zones. This idea is captivating on account of its simplicity; and I, for one, would not refuse to adopt it, if the accuracy of the analogy, were but fairly established. But as this appears to be an impossible task, I shall proceed immediately to state my objections to the supposed constitutional connexion of the birds under consideration, and the animals with which they are compared.

Analogy from torpid animals.

Those quadrupeds, reptiles, and insects, which pass the winter in a state of insensibility; may be recalled to sensation and action at pleasure, by the application of a gentle degree of warmth. This constitutional singularity of these animals, has induced philosophers to conclude unanimously, that the return of the sun in spring rouses them from a torpid condition, at a time when the benefits of the season are ready for their enjoyment. There is another circumstance, which gives something more than plausibility to the supposition when it is properly understood. For the animals in question take up their winter quarters, some of them in subterranean habitations, a little below the surface of the soil: others lodge in the crevices

vices of walls or rocks; and a few, such as frogs, female toads, and water newts, bury themselves in the mud of shallow ponds. These retreats are of all them but slightly covered by a thin stratum of earth, or a sheet of water of a moderate depth; in consequence of which, they are warmed in due season by the rays of the sun, after he has entered the northern half of the ecliptic. The preceding assertion, is not a plausible conjecture built upon probabilities; but a fact, which has been determined by experiment; for the Rev. Dr. Hales, in the course of his experimental enquiries into the process of vegetation, discovered that a thermometer, the bulb of which was buried 16 inches below the earth's surface, stood at 25° of his scale in September, at 16° in October, and at 10° in November during a severe frost; from which point it ascended again slowly, and reached 23° in the beginning of April (old style). Now the latter part of September and the whole of October is the season in which the bat, the hedgehog, the shrew, the toad, and the frog are seen but seldom, and finally disappear. The same animals all leave their retreats and are observed abroad again in the time betwixt the vernal equinox and the middle of April; which circumstance makes the preceding theory agree very well with the variations of temperature, that take place in the winter habitations of those animals, which are actually known to pass the cold season in a torpid condition.

Their retreats are shallow.

Facts stated.

After making the foregoing remarks on torpidity, I come to certain facts, which are far from favouring the supposed analogy of those animals which are known to be lethargic in winter, and our summer visitors of the feathered tribe. Birds of this description are very numerous in this part of the world at the time of their disappearance; from which circumstance it is reasonable to conclude, that if they take up their winter abode near the surface of the earth, they would be frequently found in the cold season; which is the case with bats, field-mice, and hedgehogs. Though discoveries of this kind are mentioned by various authors, the uncommonness of the circumstance obliges the advocates of torpidity to dispose of the periodical birds during winter, in places which are inaccessible to men, such as the vaults of profound caverns, or the bottoms of deep lakes. My objections to this opinion, are derived from certain facts

Birds though numerous are not found in the torpid state.

facts respecting the temperature of places situated at great depths below the surface of the land and water.

Remarks on the temperature of deep caverns it does not vary.

Every place on the globe has an invariable temperature peculiar to itself, which cannot be found at less than 80 feet below the external soil. Mr. Boyle kept a thermometer for a year, in a cave which was situate under a roof of earth 80 feet in thickness; and found that the liquor in the instrument remained stationary all the time. In compliance with my request, the late Dr. Withering made a similar experiment on a well 84 feet deep, at Edgbaston, near Birmingham, the temperature of which was found to be 49° in every month of the year 1798. Pits or wells of a less depth give more or less annual variation of temperature, according to the distance to which they penetrate the superficial strata of the earth. A remarkable singularity, however, is observeable in experiments made on pits of a moderate depth. I kept a monthly account of the temperature of a well, for the year 1795 and 1798, the perpendicular depth of which was twenty feet; and the annual variation of its temperature fell a little short of 4°. But the following circumstance deserves to be carefully remarked on the present occasion. The temperature of the ground, at the distance of twenty feet from the surface, is at the highest in October, when a thermometer exposed to the atmosphere makes the monthly mean coincide with that of the year: on the contrary, the subterranean temperature does not arrive at a *minimun* before the end of March; which is three months later than the coldest weather above ground.

Hence the lethargic animals do not retire to great depths.

The facts just stated throw much light on the subject of the present essay, by pointing out the reason which determines animals of known lethargic habits to form their winter retreats near the surface of the ground. This choice exposes them, according to the experiments of Dr. Hales, to a variable temperature, which sinks slowly at first, and keeps them benumbed by a sleepy torpor; but after the rigours of winter are past, the hiding places of these slumberers are gradually warmed by the returning sun, which reanimates their torpid limbs, and recalls them from their secret dens, at the proper moment for their appearance above ground. Had the hedgehog, the field mouse, &c. made a contrary choice, and retired to caverns 80 feet deep, all the benefit they could have derived from

from an invariable temperature, would have consisted, in the certainty of not being frozen; for the same degree of cold which disposes them to sleep in autumn, would evidently perpetuate their slumbers in these situations; unless we suppose them to be roused to action by the calls of hunger, which is a precarious and treacherous cause. For the sense of want would not fail in many instances to invite these animals to certain death in the midst of frost and snow, at an earlier season than the commencement of spring. If we suppose our known sleepers, or any other animals suspected of torpid habits, to retire to a depth less than 80 feet, but to a distance from the surface which is sufficient to conceal them, in damp and dreary grottos, from human observation; the supposition will not remove the difficulty. For the time when our periodical quadrupeds, birds, and reptiles disappear, coincides with the *maximum* of temperature in such places, and they are seen abroad again when the same temperature is at the lowest.

Birds are never found in the shallow retreats, and the deeper are unfit.

Very few arguments will be now required to demonstrate the impossibility of the analogy which is supposed to connect the periodical birds of summer, and the sleeping animals of winter. It is sufficient barely to remark, that the former are never found slumbering with the latter; near the surface of the earth; and deep caverns are proved to be unfit for the reception of any creature in the torpid season. Consequently the birds in question, desert the temperate zones at the approach of winter, to seek a better climate in lower latitudes.

The migration of birds is well established.

The migration of our summer visitors being established upon authentic facts, I intend to proceed in the next place, to give a theory of their annual motions derived from natural causes. All the birds constituting the migrating tribe feed upon insects, which disappear and become torpid, either in a perfect state or under the form of embryos, soon after the autumnal equinox. This circumstance refuses the animals under consideration a farther supply of proper aliment in the higher latitudes. They are therefore compelled, by the apprehension of starving, to use their wings and retire southwards into more genial climates, where the rigours of winter do not lock up the sources of their natural food. The manners of the winter birds of passage favour the last conclusion; for the jack-snipe, the red-wing, the woodcock, and the fieldfare, with some other species,

They follow their food; viz. insects which disappear during cold.

cies, quit the frosty regions of the north at the approach of cold weather, and spend the winter in the more temperate parts of Europe. But the return of spring admonishes them when to leave these countries; and they retire generally before the end of April, to pass the breeding season on the confines of the arctic circle. The twite (Fringilla montium) breeds on the hills of Yorkshire and Westmoreland, but does not remain all the year in its summer habitation. For twites congregate in multitudes about the beginning of October, and disappear; but large flocks of them are seen at that time, or not long after, in the south of England. Thus are the two retreats of this migrating finch pretty well ascertained. But the same cannot be generally affirmed of those birds which retire from Britain in autumn. The swallow, however, is now known to winter in different parts of Africa; and, in all probability, future observers will discover the southern retreats of the other migrating species, partly on the same continent, and partly in the warmer countries of Europe, or in the corresponding districts of Asia. The last opinion must be received as a conjecture, but it has the recommendation of being probable; because those birds which return hither about the time of the vernal equinox, may be expected to pick up a livelihood near home during the preceding months, without accompanying the swallow to the mouth of the Senegal, in the 16th degree of north latitude. Finally, we may conclude, apparently with safety, that no bird retires in autumn farther from its summer residence than necessity requires, and that its winter abode is fixed by the article of food, which depends on the temperature of the place, and the appetite of the visitor.

Habits of various birds.

They return with summer, which affords their food.

After making the foregoing imperfect remarks on the southern retreats of the migrating tribe, I come in course to the cause which invites these wanderers northward, to spend the summer in higher latitudes. No sooner has the sun touched the tropic of Capricorn, than he begins to lessen his southern declination, and to shine more directly upon the opposite hemisphere: every latitude of which experiences his animating influence in succession, commencing with the parts contiguous to the torrid zone, and proceeding gradually to the frozen regions within the arctic circle. The advances of spring towards the north, keep pace

pace with the diffusion of solar heat over the northern half of the globe. For the same plants flower much earlier in low than in high latitudes; and we may safely conclude that the same lethargic animals, especially the same flies and other insects, will observe the like rule in quitting their winter quarters; and will appear abroad in Italy much sooner than in Britain. The following comparative facts may serve to elucidate the slow progress of spring from the south to the north. I am sorry, that the observations are chiefly confined to the vegetable kingdom. The table, however, contains a remark, which is of importance to the present subject. For it traces the nightingale, a feeble bird of passage, through 22° of north latitude; by assigning the times of its appearance on three distant parallels. Now it has been shewn, that the periodic birds do not remain torpid through winter, in those countries which they frequent in summer; consequently, we may infer with safety, that the nightingale travels leisurely towards the arctic circle during the vernal months, after leaving its winter retreat, which is unknown. In this long journey, this bird passes from one degree of latitude to another, as the advances of spring prepare the successive climates of the northern hemisphere for its reception, by warming the ground, and calling the insects of each country progressively into active existence.

Facts relative to the progress of spring.

Birds migrate leisurely from clime to clime.

The Progress of Spring shewn by the Time of flowering of the same Plants in different Latitudes.

Name.	Athens, lat. 37° 25′	Stratton, lat. 52° 45′	Kendal, lat. 54° 26′	Upsal. lat. 59° 36′
Leucoium vernum. f.	Feb. 1	———	———	April 13
Narcissus pseudo-Narcissus. f.	Feb. 5	———	Mar. 22	May 15
Anemone nemorosa. f.	Feb. 14	April 10	———	———
Ulmus campestris. v.	Feb. 16	April 10	———	May 15
Cratægus Oxyacantha. v.	Mar. 2	———	April 15	May 15
Nightingale sings.	Mar. 24	April 22	———	May 15

This

From the slowness of migration, it may be performed by feeble birds.

This deliberate manner of travelling relieves the theory of migration from one of its principal difficulties. For this supposition makes an easy task of a long journey to those birds of passage which are not remarkable for agility and power of wing; such as the red-start, the yellow-wren, the nightingale, and other species. These wandering birds are not required by the theory, to fly with the greatest expedition through 40 or 50 degrees of latitude, from their winter quarters to their summer haunts. On the contrary, one of them has been proved to move slowly from one station to another, as the sun advances in his return towards the tropic of Cancer. The winter labours of the jack snipe, which is remarkable for its inactive habits, confirm the foregoing supposition. For this bird quits the northern regions early in autumn: and, in spite of its natural feebleness and indolence, makes a shift to travel over the greatest part of Europe in the cold season. The woodcock also, after leaving the same summer retreats, makes a similar journey, and passes over into Africa.

Vernal course of the swallow.

I shall now proceed to give a few points in the vernal course of the chimney swallow (*Hirundo rustica,*) which is known to travel in the spring from Senegal, in latitude 16° north, to Drontheim, in latitude 64° north. This bird appears in the neighbourhood of Senegal on the 6th of October; and has been seen as late as February in the same country. It is said to arrive at Athens, in lat. 37° 25′. on the 18th of February; at Rome, in lat. 41° 45′, on the 22d of the same month; at Piacenza, in lat. 45°, March 20th, A. D. 1738; at Tzaritzin, in lat. 48° 30′, April 4th; in the late spring of 1793, at Catsfield, lat. 51°, April 14th, from a mean of twenty observations; at Stratton, lat. 52° 45′, April 8th, from a mean of twenty observations; at Kendal, lat. 54° 20′, April 17th, from a mean of twenty-three observations; at Upsal, lat. 59° 30′, May 9th, from one observation.

This route of the swallow towards the arctic circle, shews that the bird does not rely on its agility, and loiter in the torrid zone longer than is necessary. On the contrary, it travels slowly from climate to climate, until the sun is in 17 or 18 degrees of northern declination, and spring has made considerable advances in the ungenial climate of Sweden. One anomaly occurs in the vernal progress of the swallow, which deserves the attention

attention of the naturalist, because the circumstance, when properly understood, shews how attentive the bird is to the local causes, which retard the spring in certain districts. The swallow appears upon an average, six days earlier at Stratton in lat. 52° 45′, than at Catsfield in lat. 51°. There is little or no doubt that this apparent exception to the present theory arises from some circumstances which retard the increase of the vernal temperature at Catsfield; and make the spring advance more quickly at Stratton. As I am unacquainted with the situations of both places, it will be proper to state a few facts, which shew how powerfully causes of this sort influence the excursions of migrating birds. 1st. The bank martin (*Hirundo riparia*) is commonly seen at the mouth of the river Kent six or seven days before it arrives at Kendal, though the distance does not exceed five or six miles. But the town lies near the mountains; and the air is colder in that part of the valley than at the head of the estuary. 2d. I have frequently heard the red-start, the yellow-wren, and the white-throat, singing in the gardens at Kendal, two or three days prior to their arrival at Middleshaw. I attribute this difference to the same cause; for Middleshaw lies 200 feet higher than the town, being distant from it three miles to the south east. Lastly, the chimney-swallow was seen at Kendal on the 24th of April, A. D. 1808; but did not make its appearance at Settle, before the first of May. The latter town lies south of east thirty miles from the former, in a mountainous district not far from the source of the Ribble.

Local circumstance, greatly influence the progress of birds.

The preceding instances, with other facts of a similar nature, shew how absolutely the motions of the birds under consideration, are regulated in the vernal months by local causes affecting local temperature; and the principal object of the present essay may be called an attempt to demonstrate, that the same leading cause, naturally connected with the article of food, compels them to traverse the temperate zone, wholly or in part, twice in the course of the year. When the phænomena of migration are considered in this way, winter and summer birds of passage become relative terms, belonging to the place of observation. For instance, the twite inhabits the southern parts of Britain during the cold months, but returns to the hills of Yorkshire in spring; and if we may judge from the

Recapitulation. The passage of birds is seen upon scales of different magnitude.

opposite

opposite climates of the torrid and frigid zones, the former will have no visitors but in winter, and the latter none excepting in summer. The intermediate space on the surface of the globe is the chief scene of their operations. It is here that the temperature of the atmosphere undergoes great variations, but never arrives at extremes; in consequence of which, every wanderer of the feathered tribe has the power of selecting a summer residence in the temperate zone, which is agreeable to its feelings and appetite. The different kinds of these birds can naturally subsist in places where the spring has made less or greater advances; for the red-start precedes the swallow, and the swallow precedes the cuckoo. This is the reason why the different species travel in distinct parties, resembling the legions of a numerous army marching in the same direction; the whole body being in motion together, alternately to the north and south. I shall close the essay with a table exhibiting the order of this procession in Westmoreland. The first column contains the names; the second gives the times of migrating northwards, which is when the winter birds depart, and the summer visitors arrive; the third gives the times of migrating southwards, that is, when the summer birds depart, and the winter visitors arrive.

TABLE.

TABLE.

Migration of birds.

Birds.	Migrate.		
	North.	South	
Anas Cygnus........		Jan. or Feb.	In hard frosts
Fringilla montium....	March 1	October 4	
Anas Anser.........	March 8	Sept. 10	
Numenius Arquata..	March 10	Sept. 9	
Tringa Vanellus......	March 13		
Motacilla flava......	March 21	October 24	
Sylvia Hippolais.....	March 26		
Motacilla Boarula.....	April 4		
Scolopax rusticola.....	April 8	October 14	
Hirundo riparia.......	April 12		
Turdus pilaris.......	April 14	October 18	
Sylvia Phœnicurus....	April 14 in exposed situations.	October 3	
Sylvia Trochilus......	April 15		
Hirundo rustica.......	April 17	Sept. 25	
Tringa hypoleucos....	April 22		
Sylvia Sylviella......	April 26		
Cuculus canorus.....	April 27		
Hirundo urbica......	April 29		
Sylvia rubicole......	May 1		
Charadrius Morinellus.	May 2		A.D. 1792.
Sylvia cinerea........	May 2		
Hirundo Apus.......	May 3	August 18	
Sylvia sylvicola......	May 13		
Sylvia hortensis......	May 15		
Sylvia salicaria......	May 17		

XI.

Letter from Mr. WILLIAM CLOSE, giving a Description of the Means of causing his hydraulic Apparatus to act with a variable Quantity of Water.

To Mr. Nicholson.

Dalton, June 4, 1813.

SIR,

IT has long appeared to me, that the only thing wanting for the completion of the hydraulic apparatus, the description and drawing of which you have done me the honour to insert in the 12th vol. of your Journal, is to make it operate with a variable quantity of water.

This, I conceive, may be done by having several pipes to convey air to the bell B. of the apparatus, pl. I. fig. 2, whose orifices open at different heights in the cistern, and which are supplied with air at some depth below the surface of the water.

With this view, let each pipe be contracted to half its diameter, at about the depth of one foot below the surface of the water in the cistern, and then gradually widen to its general width, and let several small holes be made in its side, at the contracted part.

Now, when water descends in these pipes, air will enter at the small holes, and the operation of the apparatus in raising, will be in proportion to the number of pipes in action.

A circular guard must be fixed on each pipe, at some height above the contraction, to prevent any water from coming at the air holes.

I set up an apparatus, such as delineated in fig. 1; but with the alterations which have been mentioned. The general width of the pipe A. B was three quarters of an inch in diameter, and about twenty-seven feet long. The other pipe was one inch in diameter—the fall of water four feet.

Upon trial, one pint of air descended into the bottle above

C. in the course of each minute, with about twenty pints expenditure of water from the cistern.

The small syphon must have an overflowing vessel to itself.

I am,

Sir,

Your most obedient, humble Servant,

W. CLOSE.

SCIENTIFIC NEWS.

Geological Society.

June 4, 1813.

The President in the Chair.

The Duke of Devonshire,
John Whishaw, Esq. of New Square, Lincoln's Inn,
Henry Drummond, Esq.
Charles Price, M. D. Fellow of Wadham College, Oxford,
William Lowndes, Esq. of Somerset Place,
Viscount Kirkwall, M. P.
Alexander Sutherland, M. D. of Great George Street, Westminster,
George Wilbraham, Esq. of Upper Seymour Street, Portman Square,
were severally elected members of the Society.

An account of the Isle of Man, by J. F. Berger, M. D. M. G. S. was read.

The length of the Isle of Man, from N. E. to S.W. exceeds thirty English miles, and its breadth varies from eight to fifteen miles. About five miles from the northern extremity a mountainous tract commences running parallel to the eastern coast of the island, and also forming the small detached island called the Calf of Man, situated at the southern extremity of the larger one. This belt, or chain of high land is divided by three transverse vallies, of which two are situated in the larger island, and the third forms the strait that separates the one island from the

the other. The highest mountains are situated in the northern division, the most elevated of which, called Snei eldt, is 2000 feet above the level of the sea.

The rocks of which this country is composed, belong chiefly to the transition class of Werner. Small grained granite occurs only in one or two places, and at an elevation of not more than three or four hundred feet above the sea. Gness and mica slate appear to be entirely wanting, as also are the oldest members of the clay slate formation. The newer portion of the clay slate formation occupies the most elevated parts of the Island, where it appears under the form of hone slate, roofing slate,

From these rocks the passage to the transition class takes place by insensible degrees; and of this the oldest member that presents itself is Grey-wakke. The trait occupied by this latter rock is, for the most part, less elevated than that where the clay slate makes its appearance and incloses it. The beds dip south, more or less, in the east, and this inclination varies from vertical to about 35°. In this formation occurs Grey-wakke, Grey-wakke slate, and granular quartz, slightly micareous, in none of which rocks are any organic remains to be perceived.

The preceding formation is covered by a deposit of limestone, less elevated above the sea than the Grey-wakke, and an inclination approaching nearer to horizontal. It consists of beds of shell limestone, resembling that of Kilkenny, and of Westmoreland, Cumberland, and Durham, together with magnesian limestone, sometimes in separate beds, and often in distinct patches enclosed within the other. This magnesian limestone, except in a single instance, appeared destitute of organic remains, but in some places encloses roundish nodules of glassy quartz.

In one or two places the limestone is covered by an unstratified mass of transition amygtaloid; the base of which is a greenish wakke, containing nodules of lamellar calcareous spar, invested by a thin coating of iron pyrites.

Of the Floetz, or secondary rocks, the only one that occurs is the oldest sandstone, some of the beds of which are so coarse grained, as to merit the name of Conglomerate, in which case it consists chiefly of fragments of quartz, with a few scraps of of decayed slate, and a little iron pyrites. The colour of the sandstone is red or greyish white; it is more or less slaty, according to the proportion of mica that it contains; it lies unconformably over the Gran-wakke, and dips N. W. at an angle varying from 35° to 15°.

On the sea shore, and on the slopes of several of the mountains, are loose blocks, in great abundance, of granite, of mica slate, and of porphyry.

The only veins in the island are at Laxey, at Foxdale, and at Brada-head. At present, however, they are all abandoned. The ore is galina, mixed with pyrites, and with the carbonates of lead and of copper. The rock through which the veins run is Grey-wakke; but at Foxdale they have been followed into the subjacent granite.

The paper is terminated by two tables. Of these the first is a register of the temperature of several springs, ascertained during the month of June, 1811. From this it appears, that the mean temperature of the island is 49° 99′, exceeding that of Edinburgh by about 2° 2′, and inferior to that of London by about 1°.

The second table contains the elevation of 78 different spots in the island, deduced from barometrical observations. Of these there are twenty-one, the height of which is between 1000 and 2000 feet above the level of the sea.

June 18, 1813.

Sir Henry Englefield, Bart. Vice Pres. in the chair.
The Rev. Edward Honey, Fellow of Exeter College, Oxford,
The Rev. George Barnes, Fellow of Exeter College, Oxford,
John Hanson, Esq of Bloomsbury Square,
John Forster Barham, Esq. M. P. of Queen Anne Street,
Thomas Bigge, Esq. of Brompton,
Samuel Turner, Esq. of Nottingham Place,
were severally elected members of the Society.

A letter from James Curry, M. D. M. G. S. was read.

In this letter Dr. C describes a remarkably large specimen of nodular agate (exhibited before the Society) which he conceives to point out a natural connexion between agate and the Plasma of the ancients.

The

The reading of Mr. Webster's paper "on the fresh-water formations of the Isle of Wight, with some observations on the strata lying above the chalk in England," was begun.

The observations in this paper were in part suggested by the recently published memoir of MM. Cuvier and Brongniart concerning the strata in the vicinity of Paris, in which they have described two marine, and two fresh-water formations alternating with each other, the whole lying above the chalk, which latter rock has hitherto been very generally considered as one of the most recent deposits.

It is to Sir Henry Englefield that we are indebted for the first observation of highly-inclined strata of chalk in the Isle of Wight.

A circumstance so material for the theory of the formation, or of the revolutions undergone by the more recent strata of the earth, demanded a leisurely and careful survey, which was entrusted by Sir H. Englefield to the well known accuracy of Mr. Webster.

The present paper is the result of this enquiry.

An elevated ridge of hills runs through the Isle of Wight, in a direction nearly E. and W. from Culver cliff to the Needles. These hills are composed of strata sometimes nearly vertical, but generally forming an angle with the horizon of from 60° to 80° dipping northward. These strata consist of the upper and lower beds of chalk, that is the chalk with and without flints, covering the chalk marl; and these again are underlaid by calcareous sand-stone, with subordinate beds of chert and lime-stone, clay and carbonized wood. To the north of these strata, occur at Allum Bay, other vertical beds of sand and clay, one of which corresponds in its fossils and other characters with the blue clay, containing septaria, usually known by the name of the London clay.

The whole series of vertical beds, exhibits no marks of partial disturbance, but it is evident, from the occurrence of these very same beds in other parts of the country, in a nearly horizontal position, and from the impossibility of some of them (consisting of loose sand with waterworn nodules of flint) being deposited in the vertical position in which they are at present, that the whole mass must have been bodily raised or depressed,

depressed, by some unknown force applied to them subsequently to the formation of the bed of London clay.

If a line in the direction of the central ridge of the Isle of Wight, be extended westwards with Dorsetshire, it will be found to coincide nearly with the direction of a ridge running from Handfast point to Lulworth, and with that already described, and which therefore may be considered as a continuation of this former.

The nearest tract of chalk to the north of this ridge, is the South Downs, the strata of which, together with their superimposed beds up to the London clay, dip gently to the south. Hence the space between may be considered as a great basin or hollow, occasioned, probably, by the rupture and subsidence of strata originally horizontal.

Within this basis, at its southern edge, that is, on the northern coast of the Isle of Wight, occurs a large mass of horizontal strata, in many parts visibly resting on the edges of the elevated strata above-mentioned, and, therefore, belonging to a period subsequent to that in which the formation of the basin took place. This horizontal deposit differs in its geological situation, in its mineralogical characters, and in the fossils which it contains, from any others that have hitherto been discovered in England, but remarkably corresponds in many of its members with the beds found in the basin of Paris, and recently described by MM. Cuvier and Brongniart, authenticated specimens of which, sent by the latter of these gentlemen to the Count de Boumon, have been by him deposited in the cabinet of the Geological Society.

These beds, as they appear in the Isle of Wight, constitute four formations; the first of which, is the lowest fresh water formation; the second is the upper marine formation; the third is the upper fresh water formation, and the fourth, or superficial, is an alluvial bed.

The particulars of these are described in the subsequent part of Mr. Webster's paper, which has not yet been read before the Society.

Account of Books, &c.

Memoirs of the Literary and Philosophical Society of Manchester. Second Series. Vol. II. octavo, 484 pages, with nine copper plates. London. 1813.

This volume contains the following papers. 1. An account of some experiments to ascertain whether the force of steam be in proportion to the generating heat. By John Sharpe, Esq. 2. On respiration and animal heat. By Mr. John Dalton. 3. An Inquiry into the Principles by which the importance of Foreign Commerce ought to be estimated. By Henry Dewar, M. D. 4. Remarks on the Rise and Origin of Figurative Language. By the Rev. Wm. Johns. 5. On the measure of moving force. By Mr. Peter Ewart. 6. Account of a remarkable effect produced by a stroke of lightning. By Matthew Nicholson, Esq. with remarks by Mr. Henry. 7. Theorems and Problems, intended to elucidate the mechanical principle called *vis viva*. By Mr. John Gough. 8. On the Theories of the excitement of Galvanic Electricity. By William Henry, M. D. F. R. S. 9. Cursory remarks on the mineral substance, called in Derbyshire, rotten stone. By William Martin, F. L. S. &c. 10. On national character. By Thomas Jarrold, M. D. 11. Observations on the ebbing and flowing well at Giggleswick in the West Riding of Yorkshire; with a theory of reciprocating fountains. By Mr. John Gough, (see our present number.) 12. Description of an endiometer and of other apparatus employed in experiments on the gases. By William Henry, M. D. F. R. S. &c. 13. A demonstration of Lawson's Geometrical Theorems. By the late Rev Charles Wildbore 14. Remarks on the summer birds of passage, and on migration in general. By Mr. John Gough.

I shall give extracts or abridged accounts of some of these memoirs hereafter.

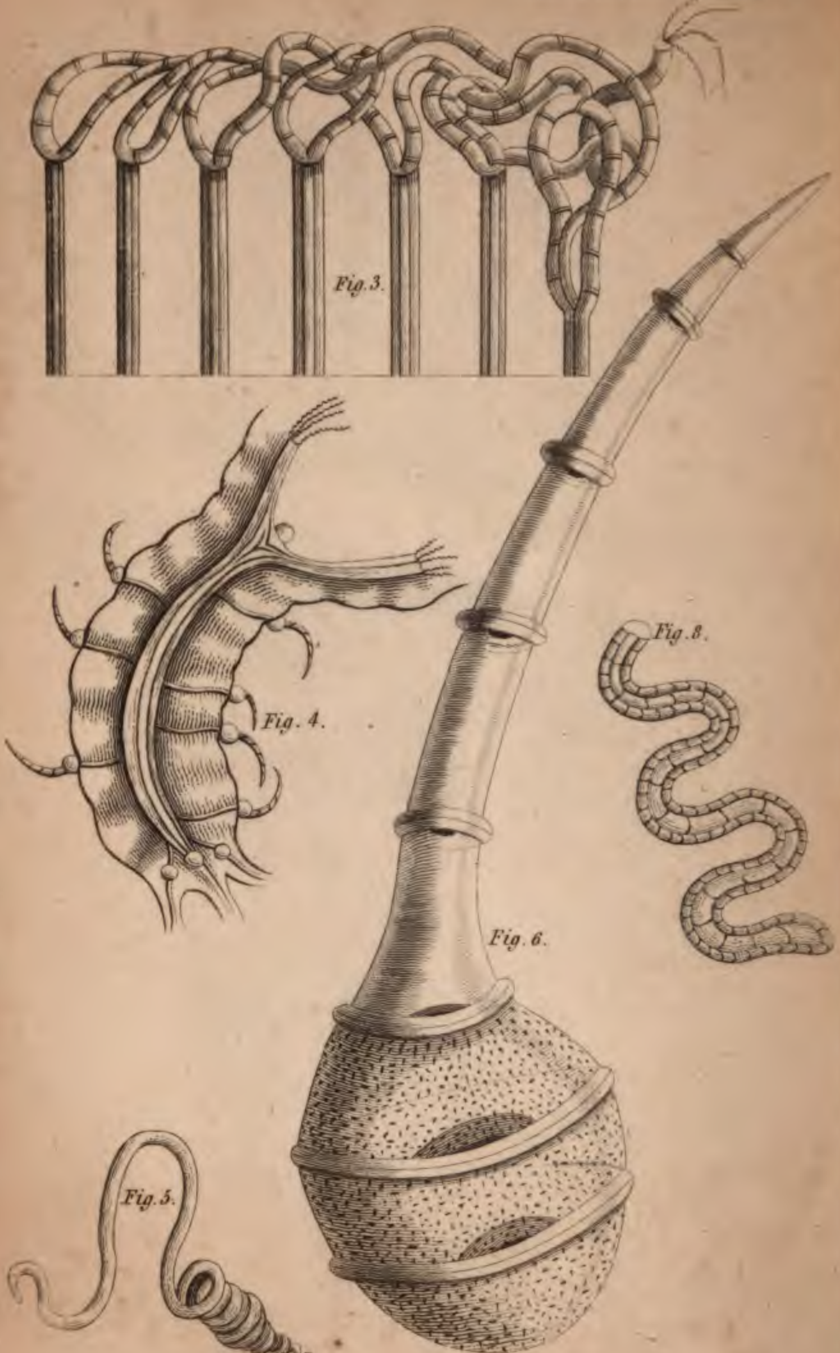


Fig. 3.
Fig. 4.
Fig. 8.
Fig. 6.
Fig. 5.

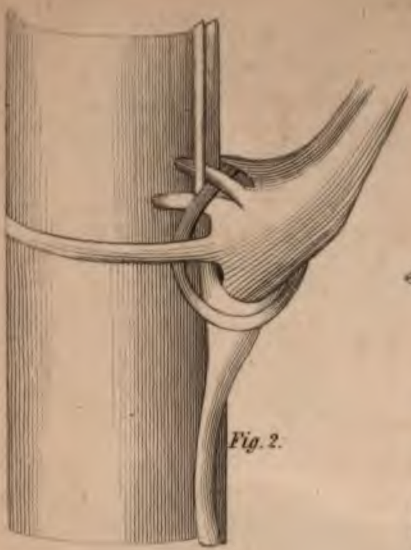


Fig. 2.

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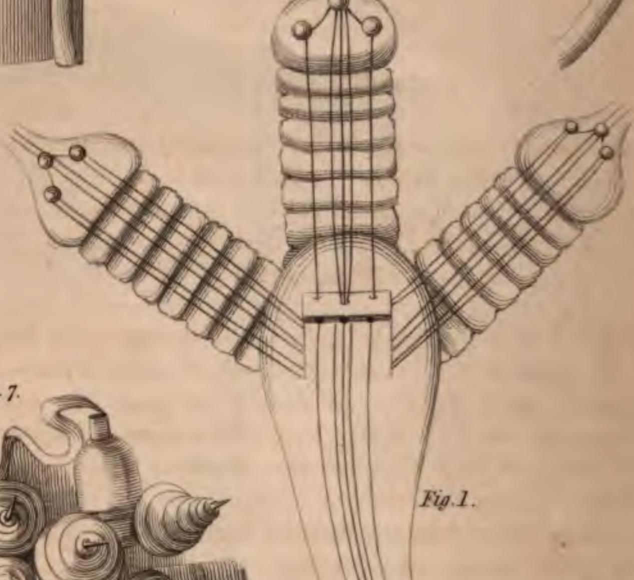


Fig. 1.

Fig. 7.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

AUGUST, 1813.

ARTICLE I.

A Theory of the Tides, including the Consideration of Resistance. By a Correspondent, E. F. G. H.

(*Concluded from p.* 159.)

THEOREM K.

THE oscillations of the sea and of lakes, constituting the tides, are subject to laws exactly similar to those of pendulums capable of performing vibrations in the same time, and suspended from points which are subjected to compound regular vibrations of which the constituent periods are completed in half a lunar and half a solar day. Oscillations of seas and lakes.

Supposing the surface of the sea to remain at rest, each point of it will become alternately elevated and depressed in comparison with the situation in which it would remain in equilibrium, its distance from this situation varying according to the regular law of the pendulum (See Theorem F): and it will be actuated by forces indirectly dependent on, and proportional to this distance, so that it may be compared to a pendulum remaining at rest in the vertical line about which its point of suspension vibrates, and will consequently follow the motion of the temporary horizon in the same manner as the pendulum follows the vibration of its point of suspension, either with a direct or retrograde motion according to circumstances : the operation of the forces concerned being perfectly analogous, whether we consider the simple hydrostatic pressure depending on the elevation, or the horizontal pressure derived from the inclination of the surface, or the differential force immediately producing elevation and depression, depending on the variation of the horizontal pressure, and proportional to the curvature of the surface. We have only to determine the time of spontaneous oscillation (ϖ) either in the open sea, or in any confined channel or lake of known dimensions, and we may thence immediately infer the magnitude of the solar or lunar tide, supposing the resistance inconsiderable; and supposing the resistance given, we may obtain by approximation a sufficiently correct idea of its effects.

Example. *Corollary* 1. Neglecting, in the first place, the resistance, we may suppose a lake or sea to be contained between opposite coasts in the direction of the meridian, and call its breadth in the direction of the equator b, and its depth d, both in miles; then the time required for the complete oscillation of such a lake will be $\frac{b}{140\sqrt{d}}$ in hours: and the square of this time will be to the square of half a solar day (☉) as $\frac{b^2}{19600d}$ to 144, or as b^2 to $2830000d$, :: $1 : n$ (Pr. A. Sch. 2.) and $n = 2830{\cdot}000\, d : b^2$, and $\frac{n}{n-1} = \frac{2830000\, d}{2830000d - b^2}$, which is the multiplier for determining the excursion of the pendulum from that of the point of suspension, or the true height of the tide from the variation of the form of equilibrium : so that if b be considered as a circular arc, the height at the eastern and western shores will be $S\, \frac{1}{2}\, b \frac{2830000d}{2830000d - bb} \cdot h$, h being the whole height of the primitive solar variation, and in the same manner taking half a lunar instead of half a solar day, we have $S\, \frac{1}{2}\, b \frac{3030000d}{3030000d - bb} h$,

or

for the lunar tide, h being the primitive lunar variation : and for a lake of 90° in breadth, where $b = 6216$, or for the open ocean, the heights become $\frac{d}{d-14}h$, and $\frac{d}{d-13}h$ respectively. It would, however, probably be more correct to make the numbers 14 and 13 somewhat larger, on account of the deficiency of velocity observable in almost all the motions of fluids.

Corollary 2. In this calculation we neglect the attraction of the parts of the sea already elevated or depressed, so that it would only be strictly accurate if the density of the sea were absolutely inconsiderable, and h were ·8 or 2 feet. But if the earth consisted wholly of a substance not more dense than water, the force tending to destroy the level of its surface would be only $\frac{2}{5}$ as great as the disturbing force which would act at the same point if the body had assumed the form of equilibrium, since $\frac{3}{5}$ of the force would be the effect of the attraction of the parts actually elevated (Theorem G.) : and the ratio of the forces would be the same in every part of the vibration : so that the time of a spontaneous oscillation would be increased in the subduplicate ratio of the diminution of the force, and the value of n diminished in the simple ratio. And if we suppose the density of the earth to be about $5\frac{1}{2}$ times as great as that of the sea, the value of n becomes reduced to $\frac{49}{55}n$, and we find for the solar tide of the open sea $\frac{\cdot 91d}{d-15\cdot 7}$, and for the lunar $\frac{2\cdot 263d}{d-14\cdot 6} = q$; and having the actual height q, $d = \frac{15\cdot 7q}{q-\cdot 91}$, or $\frac{14\cdot 6q}{q-2\cdot 263}$; the depth 15·7 and 14·6 miles being the smallest at which the tides could be direct : supposing the sea shallower, they would be inverted, the passage of the luminary over the meridian corresponding with the time of low water.

Effect of the sea's density.

Corollary 3. We may form a coarse estimate of the effect of resistance on the height and time of the tides of a given sea by considering the case of a simple oscillation subjected to a resistance proportional to the velocity. Supposing the retardation or acceleration of a lunar tide to amount to one lunar hour, the arc of the circle appropriate to the vibration becoming 30°, the

Resistance in a simple form.

the cosine of this arc will be ·866, and the height will be ·866. $\frac{2 \cdot 263 d}{d - 14 \cdot 6}$, (Th. B.) for the open sea, and $d = \frac{14 \cdot 6 q}{q - 1 \cdot 96}$: Thus if the height were 2 or -2, d would be 730 or 7·37, while the formula $\frac{14 \cdot 6\, q}{q - 2 \cdot 263}$, independent of resistance, would give only 6·45, a negative value of d being impossible. If h were -3, with this resistance, d would be 8·83. The sine of 30° being ·5, the resistance, when greatest, would be equal to half the greatest accelerating force.

Actual magnitude of the resistance.

Corollary 4. If the bottom of the sea were perfectly smooth and horizontal, we might form some idea of the resistance opposed to the tides from the phenomena of rivers and pipes; but on account of the great irregularity of form, we can only infer that the resistance must be incomparably greater than that which is thus determined. The horizontal velocity is most readily deduced from the effect of the inclination, which generates a force varying according to the law of the pendulum, and producing, therefore, a velocity, which, when greatest, is to that which would have been produced by the whole force uniformly continued for the same time, as the radius is to one fourth of the circumference: the sine of the inclination, which expresses the force, is also to the whole height divided by the breadth, as one fourth of the circumference to the radius: so that the greatest velocity becomes precisely equal to that which would be produced in the same time by a uniform force, expressed by the height of the tide divided by the breadth: and for the solar tide in the open sea, we have a force expressed by the sine $\frac{q}{6216 \times 5280}$ operating for three hours, which is equivalent to the force of gravity operating for $\frac{3q}{6216 \times 5280}$, and will generate a velocity of $\frac{345600\, q}{6216 \times 5280} = \frac{45 q}{4279} = \frac{q}{95 \cdot 1}$ in a second, or, if q be supposed equal to 1 foot, about $\frac{1}{8}$ of an inch. Now it appears from the experiments of Dubuat and others (Phil. Trans. 1808,) that the resistance may be expressed in inches of pressure by the formula $f = a \frac{l}{d} v^2 + 2 c \frac{l}{d} v$, where, for considerable depths, $a =$ ·0000413, and $c =$ ·00009, or perhaps

perhaps ·0001, d being four times the depth; but instead of having f as a measure of the height corresponding to the resistance, we may determine the equivalent inclination by finding $\frac{f}{l}$, which will be $\frac{av^2+2cv}{d}$; and this we are to compare with the greatest force tending to produce the horizontal motion, or $\frac{1{\cdot}5708q}{6216 \times 5280}$. But since $v = \frac{4147200q}{6216 \times 5280}$, $\frac{av^2+2cv}{d} : \frac{1{\cdot}5708q}{6216 \times 5280} = \frac{av+2c}{d} : \frac{1{\cdot}5708}{4147200}$, and the greatest resistance will be to the greatest propelling force as $\frac{4147200}{1{\cdot}5708}$ $(av+2c)$ to d, or as $35v+126$ to the depth in inches, that is, as $\frac{q}{2{\cdot}7} + 10\frac{1}{2}$ feet to the depth. Hence it appears that if this calculation were sufficient to determine the magnitude of the resistance, that part of it which varies as the square of the velocity would be small in comparison with the part which varies as the velocity, not only for a tide of one foot, but even for one of ten feet in height; and that both parts would become almost insensible in a sea of considerable depth. In fact, however, the observations have been made under circumstances so widely different, that no valid conclusions can be formed from them with respect to depths so great and velocities so small, even if we could disregard the irregularities of the bottom of the sea, which, by the eddies and other deviations depending on them, must create a much greater resistance than the calculation indicates; and this resistance, from the nature of the centrifugal forces concerned in it, is much more likely to vary as the square of the velocity than as the velocity simply. If we employed Dubuat's original formula $v = (\sqrt{r} - {\cdot}1)\left(\frac{297}{\sqrt{b} - \mathrm{HL}\sqrt{(b+1{\cdot}6)}} - {\cdot}3,\right)$ we might infer that the resistance or adhesion would annihilate the velocity when ${\cdot}3\,(\sqrt{b} - \mathrm{HL}\sqrt{(b+1{\cdot}6)})$ became equal to 297, b being the cosecant of the inclination, or here $\frac{6216 \times 5280}{1{\cdot}5708\,q}$; so that, if $q = 1$, ${\cdot}3\,(\sqrt{b} - \mathrm{HL}\sqrt{(b+1{\cdot}6)}) = 1369$: consequently nothing can be inferred from the calculation, except that Dubuat's formula is totally inapplicable to the case; perhaps, however, the extravagant resistance, which is indicated by it, may be admitted as a conjectural argument, to shew that the resistance,

resistance, even in a sea of a form perfectly regular, would probably be greater than is inferred from the formula for pipes and rivers, published in the Phil. Trans.

Effect of the natural resistance on the solar and lunar tides.

Corollary 5. We are next to inquire what would be the effect of a considerable resistance, varying as the square of the velocity, on the compound tide, produced by the combination of the lunar and solar forces : and the calculations in Theorem D. will serve to illustrate this case as it is found in nature. The first remarkable consequence of such a resistance is the alteration of the comparative magnitudes of the forces concerned : the extent of the oscillations being diminished by the resistance, the diminution will be greater where the resistance is greater for a given velocity, and the spring tides will bear a smaller proportion to the neap than if there were no resistance, so that the apparent inequality of the solar and lunar forces will be greater than their true inequality. We must, however, remember in making this calculation, that the proportion of the tides is by no means precisely the same with that of the disturbing forces of the luminaries, but may differ from it more or less on account of the difference of the periods, according to the depth of the ocean, and the form and magnitude of the seas and lakes concerned. For example, taking $n = \frac{2}{3}$ and $r = \frac{1}{20}$, or since $n = \frac{d}{15{\cdot}7}$, $d = 10\frac{1}{2}$ miles, the greatest resistance being supposed for the solar tide equal to $\frac{1}{20}$ of the greatest propelling force; it appears that under these circumstances, if the true spring and neap tides are generally as 2 to 1, which seems to be very nearly their true proportion, the tides which would happen if the resistance were annihilated, would be in the proportion of 4·367 to 2·055, and the primitive forces exciting these tides, instead of 6·422 and 2·312, would be $6{\cdot}422\,\frac{n-{\cdot}9344}{n}$ and $2{\cdot}312\,\frac{n-1}{n}$, or in the proportion of 5·158 to 2·312, or 2·208 to 1. It is obvious therefore, that without a more correct knowledge of the depth of the sea, and the resistances to its motion, than we possess, it is impossible to form any accurate estimate of the proportion of the solar and lunar forces from the tides, even if we suppose our observations to be exempt from the operation of any of those

local

local causes which have been described, as likely to influence this proportion: in fact it is not improbable that the irregulaties of the form of the seas are so great as to set at defiance all calculation, even if they were ascertained.

Effect of resistance on the time of high water and of spring tides.

Corollary 6. The time of high water is also subjected to various modifications, according to the resistances concerned. It is easy to see, that a resistance of any kind will produce a retardation of the direct, and an acceleration of the inverted tides, (Cor. 3); but the law of a resistance varying as the square of the velocity produces two remarkable consequences with respect to the time of high water: first, that the spring tides will be retarded or accelerated more than the neap tides; and secondly, that the highest tides will not be precisely at the syzygies, but may be before or after them, according to circumstances. The first of these consequences has not been sufficiently established by observation, although it has been remarked in general, that high tides happen earlier than lower ones, other things being equal. But in many of the harbours in which the most accurate observations have been made, the time of high water may perhaps be somewhat modified by the different resistances opposed to tides of different magnitudes in their passage from the seas in which they originate. The second circumstance is observed in a greater degree than can be well explained from the present state of the calculation. It is not easy to suppose the conditions more favourable to the retardation of the spring tides, than they have been assumed in the case stated in theorem D; and the maximum is here scarcely at the distance of a single tide from the conjunction, the second excursion being somewhat smaller than the excursion immediately preceding the conjunction, nor is it probable, that the imperfection of the mode of calculation is so great, as to afford a result very materially different from the truth. It must, therefore, remain, for the present, as a difficulty to be solved by future investigations, that in many ports not far remote from the open sea, to which the tides can by no means be, as Laplace seems to suppose, a day and a half in travelling, the third tide after the syzygy is, in general, the highest; if, indeed, this fact should

should be confirmed in all its extent by observations made in a greater variety of situations than those which have hitherto been recorded. The advance of the spring tides at the solstices, which Laplace has adduced as an illustration of the dependence of the tides on the state of the luminaries a day and a half before, is certainly favourable to his opinion. But this circumstance is not sufficiently established by continued observation, to counterbalance the incompatibility of so slow a progress of the tides from the open ocean with the well known times of high water at the different ports. There is also some difficulty in explaining the occurrence of high water at St. Helena 2¼ hours, and at Bellisle 2½ after the moon's transit, in these situations, which seem to be as little remote as possible from the source of the tides of the Atlantic. The direct tides on the eastern coasts of a sea like the Atlantic, ought to happen about an hour before, and the inverted almost *five hours* after the transit: an acceleration of 2½ hours seems to be greater than would be expected according to any probable estimate of the magnitude of the resistance. The simplest solution of this difficulty seems to be to suppose the deepest parts of the Atlantic much narrower than the whole of that ocean, so as to cause the simple inverted tide to happen considerably before the fifth lunar hour; unless we choose to consider the principal part of the phenomena of our tides as dependant on the affections of distant seas, so as to occur at times very different from those of the primitive variations of the Atlantic. The tides on the western coasts agree very well with either supposition. The slight difference of the ascent and descent of the tide, remarked by Mr. Laplace in the observations at Brest, may be explained by a comparison with the form of a common wave, which, where the water is shallow, is always steepest before. This circumstance arises from the greater velocity with which the upper parts of the wave advance, where the difference of the depths becomes considerable, (Phil. Trans. 1808); and it is, perhaps, somewhat increased by the resistance of the bottom. Where the tide travels far in shallow channels, its irregularity of inclination increases more and more; for instance, in the Severn,

Severn, it assumes almost the appearance of a steep bank. (See Nich. Journ. vol. XVIII, 1807, p. 118.)

Different mode of calculation.

Scholium 1. As a confirmation of the conclusions which have been deduced from the analogy of the oscillations of the fluids with the vibrations of the pendulums, we may obtain similar results from the immediate consideration of the progress of the tide in an open ocean following the luminary like a widely extended wave. If the actual height of the tide be called q, its virtual height, with respect to the form which would afford a momentary equilibrium, will be $q \pm h$, and the propelling force will be proportional to this height: now the natural velocity of the wave, $m\sqrt{(\frac{1}{2}d)}$, is generated by its exposure to a force proportional to its simple height for a time proportional to that which it occupies in passing over its own breadth b, that is for the time $\frac{b}{m\sqrt{(\frac{1}{2}d)}}$; and if it be exposed to a force greater or less in the ratio of $q \pm h$ to q, for the time $\frac{b}{n}$, where n is the velocity of rotation, its velocity will become $m\sqrt{(\frac{1}{2}d)}\,\frac{q \pm h}{q} \cdot \frac{b}{n} \cdot \frac{m\sqrt{(\frac{1}{2}d)}}{b} = m^2 . \frac{1}{2}d \cdot \frac{q \pm h}{nq}$, which must be equal to n, and $n^2 q = m^2 \frac{1}{2} d (q \pm h)$, and putting $n^2 = m^2 . \frac{1}{2} r$, $rq = d\,(q \pm h)$ and $q = \frac{\pm dh}{r - d}$, and $d = \frac{rq}{q \pm h}$, precisely as already demonstrated; r being the depth, affording a velocity equal to the velocity of rotation. And supposing, in the next place, such a wave to be liable to a resistance proportional to the velocity, we may illustrate the effect of the resistance by comparing it with that of another wave, which we may call its representative, combined with the original wave, and altering the inclination of its surface to the horizon. Thus, if the ordinates of the curve A (Pl. V. fig. 1) represent the elevation and the horizontal velocity of the tide, those of the curve B will exhibit the force derived immediately from the resistance, which will be the same as would be produced by the combination of the wave C with the original wave, or as if the momentary position of the virtual horizon were altered from its natural state to the form D, which is the reverse

verse of C; and this form D must be combined with the virtual variation of the horizon corresponding to the primitive tide, in such a manner as to produce a result agreeing in its position with the actual tide, and such as is represented by the curve AE for the direct, or AF for the inverted tide: and this will obviously happen if the primitive variation be represented by the space included between AE or AF and DC, the resistance GH being proportional to the sine of the displacement HI or HK, and the height of the result LE or LF, on which the true height LM immediately depends, to its cosine: the true maximum at M following that of the space AEL, and preceding that of FGH, as has already been shown by a different method.

It may be remarked, that since the resistance probably varies with the depth, the retrograde motion of the waters will be more impeded than the direct, and a very *slow*, although perhaps an imperceptible, current from east to west will thus be established, its velocity being always less than that which is sufficient to produce an equality of resistance in the different directions.

Effect of resistance on the velocity of a wave.

Scholium 2. It has been observed, in corollary 6, that the time of high water may perhaps be modified by the resistances opposed to the passage of the tides from the open ocean into the ports at which they have been observed. And indeed without a mature consideration of the subject, it would have been natural to speak with less hesitation of the effect of resistance in retarding the propagation of an undulation through a fluid. In reality, however, this retardation appears to be very inconsiderable, if it exists at all, at least where the height of the wave is moderate in proportion to that of the fluid. We may suppose AM to be the figure of an undulation advancing simply in a channel of a given depth, with a resistance proportional to the velocity, which may again be represented by the virtual elevation of the wave C, which must always accompany the original undulation AM in its progress, and must, therefore, constantly tend to produce the same motions in the particles of the fluid as a real wave of the same magnitude; for the figure of of the surface, and the velocity with which that figure successively changes or advances, are the only causes which furnish the immediate forces concerned in producing the elementary motions constituting the oscillations. Now, while the imaginary wave C advances a little into the situation N, it is obvious that, in consequence of the action of the forces which it represents, the surface must be elevated at N, where the surface of AM is depressed, and depressed in the half of the undulation next to C, where AM is elevated, so as always to diminish the magnitude of the original undulation, without affecting its velocity, as it would do if the curve C crossed its absciss in any other points than those which correspond to the greatest ordinates of AM. And the rate of diminution will be such, that if it continued uniform, the wave AM would be lowered at M during the time that it passes over one-fourth of its whole breadth AL, by a quantity which is to the greatest ordinate of C, the representative of the resistance, as the semi-circumference of a circle to its diameter: but since the resistance would vary with the height of the wave, the actual diminution would be expressed by a logarithmic quantity. Thus, if the greatest resistance be to the greatest propelling force as r to 1, the fluxion, or rather variation, of the height q will be to that of the absciss x as $90° \times qr$ to $90°$, and $-\dot{q} = rq\dot{x}$, $-\frac{\dot{q}}{q} = r\dot{x}$, and H. L. $q = C - rx$, or calling the primitive value of q unity, $q = e^{-rx}$, and $\frac{1}{q} = e^{rx}$.

E. F. G. H.

London, June 1811.

METEOROLOGICAL JOURNAL.

II.

1813.	Wind.	Max.	Min.	Med.	Max.	Min.	Med.	Evap.	Rain.	
5th Mo.										
MAY 23	W	29·59	29·55	29·570	59	49	54·0	—		☾
24	N W	29·70	29·55	29·625	60	49	54·5	—		
25	N W	29·76	29·65	29·705	61	49	55·0	—		
26	W	30·05	29·76	29·905	57	41	49·0	—		
27	N W	30·10	30·05	30·075	60	42	51·0	—		
28	Var.	30·10	29·96	30·030	72	42	57·0	—		●
29	N E	30·06	29·96	30·010	76	53	64·5	—		
30	S	29·97	29·90	29·935	73	56	64·5	·65	·34	
31	N W	30·00	29·97	29·985	78	59	68·5	—	2	
6th Mo.										
JUNE 1	E	29·97	29·83	29·900	85	50	67·5	—		
2	N E	29·95	29·83	29·890	84	54	69·0	—		
3	N W	30·17	29·95	30·060	72	51	61·5	—		
4	N W	30·17	29·88	30·025	65	51	58·0	·75		
5	N W	29·88	29·82	29·850	58	48	53·0	—		☽
6	N E	29·73	29·67	29·700	56	47	51·5	—		
7	N E	29·80	29·73	29·765	71	43	57·0	—		
8	N E	29·73	29·51	29·620	75	48	61·5	—		
9	N W	29·53	29·43	29·480	75	54	64·5	·48	—	
10	N W	29·83	29·53	29·680	75	48	61·5	—		
11	S	29·83	29·74	29·785	76	52	64·0	—		
12	S E	29·98	29·74	29·860	77	45	61·0	—		
13	W	30·02	29·97	29·995	75	51	63·0	·48	5	
14	S W	30·07	29·78	29·925	67	52	59·5	—	—	○
15	S W	29·98	29·78	29·880	67	46	56·5	—	—	
16	N W	30·04	29·98	30·015	63	44	53·5	—	—	
17	Var.	30·09	30·04	30·065	61	43	52·0	—	—	
18	N	30·15	30·09	30·120	61	39	50·0	—	—	
19	N	30·16	30·15	30·155	57	37	47·0	—	—	
20	N E	30·20	30·16	30·180	60	42	51·0	·42	·65	
		30·20	29·43	29·889	85	37	57·93	2·78	1·06	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Fifth Mo. 30. A shower p. m. Thunder to the westward. 31. Fine day: some thunder clouds appeared: the evening twilight was brilliant and tinged with orange—the new moon was conspicuous, and there fell much dew.

Sixth Mo. 1. Cumulus, cumulostratus, and cirrostratus clouds. The sunset was cloudy, with an orange tint. 2. Cirrostratus clouds, with haze, to the S. at sunset. At the same time there were cirri in the N. more elevated, and finely tinged with *red.* 4. Windy: cloudy till evening. 5. Clear a. m. afterwards cloudy and windy. 8. Windy: at sunset, cumuli, with the cirrostratus attached: much orange in the twilight. 9. A shower early: cloudy, dripping. 10. p. m. Large elevated cirri. 11. a. m. cumulostratus clouds; p. m. cirri in abundance lowering and thickening. 12. Cirri, tinged red in the morning early: before 8 it was overcast, and rain fell. 13. a. m. Cloudy: a shower at evening. 14 to 20. Occasional showers, some of which were heavy rain.

RESULTS.

Prevailing Winds northerly.

Barometer: greatest observed elevation, 30·20 in.; least 29·43 in.
Mean of the period 29·889 inches.

Thermometer: greatest height 85°; least 37°;
Mean of the period, 57·93°.

Evaporation, 2·78 in. Rain 1·06 in.

L. HOWARD.

TOTTENHAM,
Sixth Month, 22, 1813.

For the greater part of the observations on the barometer and thermometer, for the present period, I am indebted to my friend, John Gibson, of Stratford.

III.

On common Ink for writing. By Dr. BANCROFT, *F. R. S. &c. From his Researches into Permanent Colours. With additional Remarks.*

CICERO (Tuscul. 5,) has given a copious statement of the important benefits resulting to mankind from the use of ink; but probably they might, with more propriety, be ascribed to the *art* of writing, than to the *means* of exercising this art, of which ink is but one, though certainly it is not the least interesting among them.

Inks have been usually black.

The Latin name of ink, *atramentum*, and the Greek name of *writing ink*, *μελανγραφικοῦ*, strictly denote a *black* substance, and authorize us to conclude, that this was originally and exclusively the colour of the liquids employed for writing; though afterwards other coloured fluids were applied to the same purpose; and then to the names signifying *black* ink, or black matter, other words signifying *red*, *yellow*, &c. were *incongruously* united*.

The ancients knew the black from galls and iron, but not as an ink.

It appears that the ancients had not, even in Pliny's time, become acquainted with our writing ink, or at least with the use of it as such, even though the black, produced by a combination of iron with the colouring matter of galls, oak, bark, &c. was frequently brought under their observation by shoemakers, who gave this colour to their leather, as they do at present, and by the very same means; it seems probable, also, that galls were used to dye black with sulphate of iron, at the time when Pliny wrote his comprehensive work; for in his xvith book, chap. 6, after mentioning that all trees which produce acorns afford *galls*, Pliny adds, that those of the oak *hemeris*, (which he had previously mentioned as bearing the largest acorns) were the best and most suitable for *tanning lea-*

* "Verum tamquam peculiare *nigro* colori esse censeo hoc *atramenti nomen*; quanquam pro pigmentis scriptoriis singulis, et diversi coloribus usurpatum sit." Caneparius. 191.

ther;

ther; that those of the broad-leaf'd oak were lighter and less esteemed; but that it moreover produces a sort of black galls, which were more useful in dyeing;* and these he mentions again in the next chapter; and in the 4th chapter of his 20th book also; where, after describing the different sorts of galls, he adds, that they all possess similar properties, and *dye hair* or wool *black*, "Omnes capillos denigrant." That their colouring matter was combined with iron for this purpose, is not, so far as I can recollect, expressly stated any where, but that Pliny was acquainted with the colour produced by such combination, is evident from the 11th chapter of his thirty-fourth book; in which, after mentioning the salt of copper (blue vitriol) as being frequently adulterated by that of iron (green vitriol, which the Greeks call *chalcanthon*) he observes, that this adulteration might be detected by impregnating paper (that of the papyrus, with an infusion of gall nuts, and then smearing it over with a solution of the cuprous salt, which, if so adulterated, would produce a black colour. ("Deprehenditur & papyro galla prius macerato: *nigrescit* statim ærugine illita.") But the knowledge which the ancients possessed of the production of a black colour by a combination of iron with galls, oak bark, &c. is demonstrated by the use which they made of a solution of iron, to give that colour to tanned leather. This solution, or the sulphate of iron, dissolved by water for that purpose, was generally called by the name of "atramentum sutorium†," *shoe-maker's black*. The Greeks and Romans, indeed, had incorrect notions of the nature of the sulphate of iron, and supposed it to bear some relation to copper, as the moderns did long after; an error which occasioned it to be commonly called, as it is even at this time, *cop-*

Galls a very ancient test.

* "Sed gallam *Hemeris* optimam, & coriis perficiendis aptissimam: similem huic lati folia, sed leviorem, multoque minus probatam; fert et nigram: duo, enim genere sunt: *hæc tingendis utilior*."

† This was afterwards used to signify bribery, and a person put upon his trial, and corruptly acquitted, was said to be "atramento sutorio absolutus," absolved by shoe-maker's black.

peras.

peras. Thus Pliny tells us, (lib. xxxiv. cap. 12,) that the Greeks, in consequence of this supposed relation, had given the name of chalcanthum to that substance which the Latins denominated atramentum sutorium; and after mentioning how, and the places where it was produced, its colour, transparent glossy appearance, &c. he adds, that being dissolved or diluted, it served the purpose of staining leather black*.

The inks of the ancients were of coal, and not iron.

But, notwithstanding this knowledge of the black, produced by iron with galls and other matters, then employed for tanning skins, (among which were the pods of an Egyptian acacia, the cups of acorns, &c.) no application of this knowledge, for the production of *writing ink*, appears to have taken place, when Pliny wrote his history; for in his thirty-fifth book, chap. 6. after mentioning the black pigment employed by painters, and describing it as being obtained like our lamp-black, by burning rosin or pitch in places which, he says, were constructed purposely to hinder the escape of smoke, he observes, that the *best* was obtained from torch wood, or pitch pine, though frequently adulterated by the soot collected in furnaces and bagnios; *and this, he adds, is employed to write books*†. He afterwards adverts to the wonderful nature of the cuttle fish and its ink, (of which, and of the use made of it to conceal themselves when in danger, he had given an account in the 29th chapter of his ninth book;) but observes, that no use was made of it as ink. "Mira in hoc sepiarum natura: *sed ex his non fit.*" In the same chapter, Pliny adds, that, for the purpose of writing, the lamp black, or soot, was rendered much more useful by being mixed with gum, and for painting, by an admixture of blue—"perficitur librarium gummi, tectorium glutino admisto." And any person who will take the trouble of mixing lamp black with water, thickened a little by gum,

Cuttle fish.

* "Græci cognationem æris nomine fecerunt & atramento suturio: *appellant* enim chalcanthum: nec ullius, æque mira natura est." ——————" *diluendo fit atramentum tingendis coriis.*. Plin. xxxiv. cap. 12.

† "Laudatissimum eodem modo fit e *tedis*;" adulteratur fornacum balinarumque *fuligine, quo ad volumina scribenda utuntur.*"

may

may obtain an ink of no despicable quality in other respects, and with the advantage of being much less liable to decay by age, than the ink now in common use.

The ferruginous ink came gradually into use.

But though the Greeks and Romans, in Pliny's time, were acquainted with no better writing ink than that which I have just mentioned, the knowledge they had acquired of the colour resulting from a combination of gall nuts, &c. with iron, would naturally lead them to employ the black so produced, as ink; and probably after doing so, they would find motives to induce them gradually to adopt its use; as, indeed, they appear to have done, though I cannot discover from Caneparius, who has written a volume on the subject, any evidence of the time when this substitution began. We may, however, infer from Sir Charles Blagden's communication to the Royal Society, (in the Phil. Trans. for 1787) that in the *9th* century those who made it their business *to copy* manuscripts, used ink composed from iron and galls, though, probably the use of it was not so general, as wholly to preclude that of *lamp black* or soot in this way*. I shall, indeed, presently mention a composition of my own for ink, of which lamp black is a principal ingredient, and which may probably cause the latter to be hereafter employed in a similar way; at least for purposes which may require a most durable and almost *indestructible* ink.

Indestructible ink.

Account of the modern ink by Caneparius,

Of the writing inks most generally used in the beginning of the 17th century, when the Work of Caneparius was printed at Venice, he gives an account between pages 170 and 179; and they ap-

* That the use of lamp black in making ink was not wholly laid aside when Caneparius wrote, appears by the receipt which he has given at p. 176; for a *portable* ink, to consist of one pound of honey, the yolks of two eggs, half an ounce of gum arabic in powder, as much lamp black as would render the mixture sufficiently black. These were to be well beaten, and mixed in a mortar, so as to make an uniform and solid mass; of which, when wanted, a little was to be dissolved in water. Even at this time, the Boors at the Cape of Good Hope are said, I think, by Mr. Barrow, to write with soot and brown sugar mixed in water; and I have often seen such ink employed by farmers in North America.

pear to have consisted principally of galls, sulphate of iron, water and gum, in different proportions, to which some persons added the bark of mountain ash, or the ripe berries of the privet; and others the rinds or peelings of pomegranates; and these last he strongly commends, as contributing very much both to the *lustre* and *blackness* of the ink; and I have sometimes been disposed to adopt this opinion; but, probably, the good effects which I had occasionally observed from this addition, may have been manifested only when, from a defect, or want of the galls, the proportion of iron would have been too great without the pomegranate peels, which, indeed, will alone produce a good ink, with sulphate of iron*.

who gives the proportions.

The best proportions among those suggested by Caneparius, seem to have been half a pound of sulphate of iron to one pound of galls, with a quarter of a pound of pomegranate rinds, and about as much gum; but even ink so made, would have been more lasting, if not blacker, with a few ounces more of galls. He afterwards highly commends the addition of a little white sugar to ink. Some persons, he tells us, employed wine instead of water, which rendered their ink less liable to be spoiled by freezing; and to obviate this more effectually, Caneparius proposes an addition of brandy to the ink. He observes, at page 172, that some *ink-makers used the black liquor of the cuttle fish*, in addition to the other matters. ("Admiscent atrum liquorem sepiæ piscis marini," &c.)

Ink powder.

At page 177, Caneparius directs the composition of an ink powder, by mixing and grinding together galls, with about one-third of their weight of sulphate of iron, and one-fifth of gum, and the same quantity of alum, (which last is, I believe, now properly omitted in these compositions) and in the following page he describes an ink for staining linen, &c. which was prepared by putting iron filings and powdered galls into the strongest vinegar, and placing them over the fire, until so much

Iron water.

* Having noticed some printed receipts for making ink with pomegranate peels and sulphate of *copper*, *instead of iron*, I prepared such an ink; but the colour, as I had expected, was merely an olive brown, *not black*.

of the metal had been dissolved, as would produce the required blackness. The fluid part of the mixture was then separated by straining, and thickened by gum. This composition, though differing in regard to the method of preparing it, resembles the prosubstantive black from acetate of iron and galls, commonly employed at this time by calico printers.

Ancient writings restored by galls.

At page 179, Caneparius describes a method of restoring the blackness of writings, which were become illegible by age, and this was by an infusion of galls in white wine, which he afterwards subjected to an unnecessary distillation, and then applied the liquor to the *faded letters*, by a sponge, or a little cotton, which, he says, rendered them as distinctly visible as when first written. Prussian blue was not then known, and, therefore, the application of its colouring matter for this purpose, (as recommended by Sir Charles Blagden) was impossible; and that being the case, the means suggested by Caneparius, (excepting the distillation) were the best which could have been employed, and seem to indicate, that he must have justly imputed the loss of blackness in writing ink to the decay of its *vegetable*, and not of its *metallic* part.

Modern researches.

Though two centuries have nearly elapsed since the publication of Caneparius's work, no improvement of much importance seems to have been since made in the composition of writing ink. The late Dr. Lewis, indeed, bestowed particular attention upon this subject; and his Philosophical Commerce of Arts contains some accurate, as well as judicious observations relating to it; especially in regard to the use of logwood, with which Caneparius does not seem to have been acquainted, at least as an ingredient in the composition of ink.

Dr. Lewis.

Inquiries into the proportions of ingredients.

The desired blackness of colour, as well as its durability, in this composition, depend entirely upon the proportions in which the vegetable colouring matter, and the oxide of iron, are united; though among the different recipes which have been published, the variations are so great as to manifest either culpable ignorance in the authors of them, or great diversities in the quality of the galls, from which the colouring matter is generally directed to be obtained: in some of these recipes, equal parts of galls, and sulphate of iron are directed; and in others, six times as much in weight of the former as of the latter. Certainly galls from different species of oak, and from different countries, vary much in their comparative proportions of colouring matter; and even among those which are commonly called the best Aleppo galls, one pound of the heavy blue, or unperforated galls, will commonly prove equal to one pound and one half of the *white*, from which the insect has escaped, and which, from their having been longer upon the tree, with large perforated or open cavities, exposed to the weather, and particularly to rain, will have suffered a considerable loss of colouring matter. These two sorts of galls, as commonly imported, are mixed together in nearly equal portions, and are then called *galls in sorts*, which are to be understood as meant by me when the contrary is not expressed.

Proportions stated.

Of such galls, I think, from the results of numerous experiments, that three pounds will afford the most suitable proportion of colouring matter, for one pound of sulphate of iron, when the former is intended to be obtained exclusively from galls; and when logwood is to be employed conjointly with the latter, the galls may be diminished at the rate of one half of the weight of logwood. In regard to the proportion of galls to that of sulphate of iron, my opinion accords with that of Lewis, who found that three pounds of the former to one of the latter, commonly produced the best and most lasting ink. Ribaucourt, indeed, thinks two pounds of galls sufficient for one of sulphate of iron, and certainly with this proportion an ink may be produced sufficiently black; but not so durable as it would be with a larger proportion of vegetable colouring matter.

In regard to the use of logwood, Chaptal does not consider it as being capable of adding any thing to the blackness of ink, made with galls and sulphate of iron, in suitable proportions; but he thinks that it contributes to hinder a precipitation of the colouring matter, and that the ink, of which it is a component part, is, by its use, rendered more smooth, or *marrow-like* (moëlleux) and the black in appearance more soft; and that

that the strokes made with it by the pen are more clean. To me, however, logwood has always seemed to give additional body or fulness to the colour of ink, though it cannot be supposed to render it more lasting; for, by many experiments, I have found, that neither on paper, or parchment, any more than on linen, or cotton, or, indeed, wool, was the black resulting from a combination of logwood and iron, of equal durability with that from galls and iron. And it may, therefore, be best in making ink, to employ, as Chaptal advises, only half as much in weight of logwood as of galls. He thinks, also, that the addition of sulphate of copper, in the proportion of one ounce to every fifteen ounces of galls, produces a good effect; that the bluish tint which accompanies ink when first made, even in the most suitable proportions, (until sufficient oxigene has been absorbed) will be sooner overcome by this addition, and that it will also contribute to render the ink more lasting.

But of this last effect I am very far from being convinced; because it has been fully ascertained, by experiments which I have repeatedly made, that the colouring matter of logwood cannot be made so durable upon either paper, wool, silk, linen, or cotton, when united to an *oxide of copper*, as it is with that of *iron*; and though, by producing a *dark blue* with copper, it may improve the shade of black resulting from the iron and galls, this blue, by fading sooner than the black which logwood produces with iron, (when no copper is present,) must render the ink so much the less durable. I have here supposed the effect of copper to result exclusively from its union with the colouring matter of logwood; for with that of galls it can produce neither blue nor black.

(To be continued.)

IV.

Observations on the fall of Stones from the Air, or Aerolites. *By M.* Marcel de Serres*.

History of stones which have fallen from the air,

THE phenomenon of the fall of stones is in itself so singular, that we ought not to be surprised, that although it had been observed by a number of enlightened men†, it should have been long doubted whether such an event had really taken place. The ancients, who were much more credulous than the moderns, have almost universally admitted of the fall of aerolites; but when attempts were made to account for the various terrestrial phenomena, the existence of these was totally denied, because it was scarcely possible to explain their formation.

by Albertus Magnus and others.

The first natural philosopher who, in modern times, has discussed the origin of aerolites, and proved their existence, is Albert Groot, or Albertus Magnus, whose numerous writings compose near twenty-two volumes in folio‡. But from his time until Chladni, that is to say, from the thirteenth century to our time, naturalists and philosophers have scarcely paid any attention to this phenomenon. It has, however, become necessary to admit their reality, and since this has been verified, many writers have mentioned the great number of proofs the ancients have left us of their existence§. If the writers who

* Ann. de Chimie, LXXXV. 262.

† Pliny speaks of stones fallen from the skies, as having seen them himself. *Ego vidi ipse in Vocontionum agro, paulo ante delatum. Lib. II. caput 60.*

‡ Albert Groot was born at Lawingen, in Swabia, in the year 1205. He was bishop of Ratisbon, and cultivated the sciences with some success. His history of animals is remarkable for the time in which it appeared, though the main part of the work is borrowed from Aristotle and his commentators, especially Avicenna.

§ Memoires sur les Aèrolithes, par Chladni. Journal des mines, tom. XV, XXV, et XXVI. Lithologie atmospheric par M. Izarn. Mèmoire historique et physique sur la chute des pierres, par M. Bigot de Morogues.

have

have been occupied with this department of research, may have exhausted every thing which relates to erudition on this subject, it appears, at least, that they have been unacquainted with the various opinions which have lately been made public in order to explain the formation of aerolites. Several German writers have, in fact, adopted an hypothesis on the subject of these meteors, which the French natural philosophers have not given an account of, either because the writings which contain this opinion*, have not come to their knowledge, or that the German language is but little cultivated. My present observations have no other object than to repair this slight omission; and in mentioning the hypothesis admitted by certain German writers, we cannot avoid referring, in some degree, to the work which M. Bigot de Morogues has recently published on the same subject.

Hypotheses to account for falling stones.

We may reduce to three principal hypotheses all those which have been advanced, in order to explain the production of aerolites. Some ascribe to them an extra atmospheric origin, and others, on the contrary, consider them as formed in our atmosphere; and others have been of opinion, that aerolites have their origin in the earth. But all these explanations will require to be again subdivided accordingly as the formation of the aerolites are ascribed to one or other of these causes. Thus, amongst the philosophers who have given these stones an extra atmospheric origin, we find—1st. That some, and among them Pliny, suppose them to come from the sun, from which he deduces their black colour, or rather their appearance of having been burned (calore adusto.)

That they come from the sun;

2d. That others, agreeing with Chladni, consider them as small insulated planets, or rather with M. Lagrange as fragments of small planets.

that they are planets;

3. Lastly, some with the illustrious author of the Mecanique Celeste, are of opinion, they are bodies projeced from the moon, which notion has been adopted by the greatest part of the English philosophers.

that they fall from the moon;

* Journal de Physique de Schweiger tom. V.

Those

Those observers who, on the contrary, have given an atmospheric origin to aerolites, have thought,

that they are condensed in the air;

1st. That they have been produced in our atmosphere by the combustion of inflammable gases, which contain in suspension or in solution, metallic or earthy particles.

or otherwise produced,

2d. Or they have been produced in the same manner as the metals and earths are formed in plants, as the experiments of Schrader and Crell seem to have proved. These philosophers have observed, that by causing plants to vegetate in sulphur or charcoal, the metals or earths which they usually contain, are also produced in them, under these circumstances, by the act of vegetation.

or that they have not fallen, but been changed by lightning.

As to the natural philosophers who have attributed a terrestrial origin to aerolites, some have admitted* that these substances pre-existed in the places where they were found, having only been changed by lightning, and that they proceeded from volcanos, and are a species of lava.

The opinion which tends to make aerolites be considered as formed by new combinations supposed to take place in the atmosphere, by the contact of all those bodies which evaporation incessantly carries thither, is so little known, that it is the only one to which we shall direct our observations.

The present discussion is confined to their production in the air.

The authors of this hypothesis first made the observation, that the fall of aerolites does not appear to take place equally in all seasons; for out of sixty-five or sixty-six of these falls, of which the epocha is well known, near two-thirds have happened during the months of June, July, and August. And lastly they prove still farther, that in all the winter months the fall of stones is much less frequent than in a single month of summer.

At what time of the day stones have fallen;

The same observation which demonstrates the influence of the seasons in the fall of aerolites, equally applies to the different parts of the day; for in a catalogue made with care of all falls of meteoric stones that have been known, seven only have

* This was the opinion of the members of the Royal Academy of Sciences, during the middle, and towards the end, of the last century.

fallen

fallen between midnight and noon, and even these stones have not fallen except during the more advanced hours of the morning—that is to say, from eight to eleven. Upon only one occasion this phenomenon has been observed between the hours of eleven in the evening, and six in the morning, while there exist proofs of thirty-six falls having occurred between noon and midnight, and the greatest part took place between three in the afternoon and sun-set.

and in what places;

The geographical situation of the places where these stones have as yet fallen, has not been disregarded by these observers. It seems to be a fact, that the number of these meteors decrease with the distance of the place from the equator; for none have yet been seen in Sweden or Denmark, and it is only in the southern part of Russia that four have been found; and lastly, only six are reckoned to have fallen in England. On the contrary, the number of aerolites has been very great in Italy, France, and Germany.

and in what kind of weather.

The weather appears even to have a certain influence on the fall of aerolites, for they have never been known to take place during cloudy weather, or in a heavy rain or fall of snow, or with a strong wind, especially with a north, north-east, or easterly wind. Out of forty-three falls, in which the weather has been noticed, twenty-nine happened when it was warm and calm. The thirtieth and thirty-first were seen when the sky presented some dispersed and separated clouds. The twelve others were accompanied by rather violent storms and hail, as happened at those falls which occurred in 1103, 1249, and 1552. The pressure of the air seems also to be diminished previous to and after the fall. This was observed in 1806 at Alais*, and at Stannern, in Moravia, in 1808†, and at Maurkirchen, in Bavaria, in the year 1811‡, when the heavens were obscured a short time after and before the meteor.

* Analyse de l'aerolithe tombee á Alais, par M. Thenard; Annales de Chemie, ann. 1806, p. 108.

† Journal de Physique de Gilbert, tom. XXIX. Analyse par M. Vauquelin Annales de Chemie, ann. 1809. p. 321.

‡ Journal de Physique de Gilbert, tom. XXIX.

After

Their mode of origin.

After having described the circumstances which accompany the fall of aerolites, the authors of the theory we are speaking of proceed to their origin. Of the twenty-nine stones that have fallen during calm weather, twenty of these aerolites have appeared to come out of a cloud of small extent, but of a rounded figure, and a black or variable colour, according to the colour of the stones themselves; for instance, its colour was white in the fall which was observed at Burgos, and the stones which came out of the cloud were white.

A mist or cloud is essential to the fall of stones from the air.

A mist or cloud seems to be always essential to these meteors, from which proceeds the noise which accompanies or precedes the fall of aerolites, and from which the stones themselves are emitted. The extent of these meteors is usually not less than from a half league, to a league in diameter, a size very different from that of the stones themselves, of which the mass is very frequently of small dimensions. This difference cannot be explained by admitting that the vapours give this extent to the meteor; for then the meteor would be composed of the metallic globe, and the vapours it carries after it, whereas the form of the metallic globe is always more or less round and circumscribed. It must then be supposed, that the greatest part of these globes is not composed of metallic particles alone, whilst they are passing through the air, but of inflammable particles, which are consumed during their rapid course.

The luminous globes are not merely ignited, but likewise in a state of combustion, &c.

This appears still farther proved by the luminous phenomena which accompany these meteors, for they are not the same as those produced by ignited metallic bodies. The colour of the flame is in fact white, like that of camphor or of phosphorus when ignited. In the aerolites of Connecticut or Weston, which fell in 1807*, the light followed exactly the line of the explosions, was extinguished with each explosion, and re-appeared with the succeeding one. If, as some philosophers have pretended, the light was the consequence of the ignited state of the aerolite, occasioned by its rapid fall, it would follow necessarily, that this state of incandescence should increase with the time of the

* Journal de Physique de M. Lamétherie, May 1808.

fall;

fall; but it is almost always the contrary, and several stones that have been observed in the air have been extinguished before they reached the ground. The form of these meteors also agrees very well with this opinion*, for it is not always the same; the stone that fell in England on the 18th of August, 1785, assumed at one time a round form, and at others was more lengthened out. The effervescence so remarkable, that has been observed in several of these aerolites, appears to prove still farther, that they have not the metallic consistence, nor that these are simple vapours which surround the stone, because the edges are always very distinct, and are not insensibly shaded off. In short, if contrary to all probability, the greatest part of this meteor were to be attributed to the vapours, it would be equally embarrassing to explain their origin; since the aerolites are almost entirely composed of earthy and metallic parts, which can hardly be converted into vapour at the temperature of our atmosphere.

Whether these stones move in an orbit. Proof to the contrary.

The aerolites are said to have generally moved in a parabolic orbit; but the angle which the parabola forms with the horizon is not always the same. In France, in the year 1785, a stone fell which made a hole that was nearly horizontal, and that of Stannern† formed a cavity of two feet and only two inches in depth. Other stones, like that which fell at Orleans in 1810, or that which was observed in Calabria in the year 1755, constantly preserve in their fall an almost perpendicular direction. These facts seem to indicate that besides gravity, there is some other force which, opposed to the direction of their weight, may modify their course. We have a fresh proof of this in the aerolite of Connecticut, which, before it was extinguished, and after having exploded three times, rebounded an equal number of times upwards, and consequently took a direction totally opposite to that which its weight might have caused.

Their velocity.

With regard to the velocity of aerolites, it appears in general very great; and often equals or even surpasses that of the earth's rotation; but in all these cases the acceleration is much

* See the work of Chladni, on masses of iron fallen from the sky.

† Journal de Gilbert, volume already referred to.

greater

greater than what would have been received from a simple fall. Besides, according to a great number of observations, it is quite uniform, and not augmented with the time of the passage of these stones through the air.

Duration greatly varies.

The duration of this phenomenon seems also to present great variations; for example, it varies from a quarter of a second to some minutes; one singular circumstance is the deep noise which resembles the report of cannon, and almost always accompanies the fall of aerolites, lasted in an explosion which took place in Russia in 1787* for four entire hours, that is from one o'clock till five before the stones fell. It was also observed in 1200, before the fall of stones which took place near Abdona, in Italy†, that the cloud from which the stones were precipitated as if on fire, remained visible for near two hours.

Noise of explosion.

The great differences of velocity shew that they are not moved principally by their weight.

It may be further considered as a fresh proof, that besides their weight, there is a power which influences the direction of aerolites, on remarking the small depth these stones penetrate into the earth. Long ago it has been observed, that if left to the action of their own weight, these stones ought to enter very deeply into the earth, if the moon was their point of departure, and that their velocity ought to be influenced by their volume, or their mass. This, however, is so far from being the case, that in a shower of stones, which took place 1768, in Maine, and another in 1790, in Gascony, there were several that fell with very little celerity, others very slowly, and others more swiftly; some, in short, with such rapidity, that they made a loud whistling in passing through the air; and these differences in velocity were not at all governed by their weight. Lately, one of the stones that fell at Thoulouse in 1812, of which the density was the same as usual, touched the earth so lightly that it scarcely left any impression of its fall. Some other stones likewise, that fell at Agen, had not the force to penetrate the roofs on which they rolled: this was also observed in the stones that fell in 1755 near Tabor‡.

* Journal de Physique de Gilbert, tom. XXXI.

† Izarn Lithologie Atmospherique.

‡ History of the Aerolites that have fallen in Bohemia, by Mayer, Dresden, 1805.

It

It is not less remarkable to observe, that the large and small stones do not fall together, but at the beginning of the course the largest are precipitated, and they become smaller in proportion as they approach the other extremity of the line of projection. This was observed in the meteor at Stannern, which moved from east to west, and threw down the largest stones in proportion as it advanced. The meteor which appeared at L'Aigle* and in several other places, have exhibited the same phenomena.

The larger stones fall first.

On examining the cohesion of these atmospheric stones, we find that it is not the same before as after their fall. A great number of these stones are in such a soft state, that they are frequently flattened on touching the earth, which was the case with those which fell in the years 1768, 1753, 1808, &c. Some have even been seen in a state resembling fusion or fluidity, as were those which fell at Lesay, near Constance†, in 1731, and lastly those which were picked up in Poland in the year 1796‡. But all these stones become solid, and even compact, some time after their fall. That soft state in which aerolites are often found, agrees very well with their shape, which is almost always that of a triangle or pointed figure, rounded beneath, or of an oval flattened at the lower part, a form which all bodies present when they fall from any considerable height, and do not possess great solidity.

They are soft at first and become hard afterwards.

With regard to the temperature of aerolites when they reach the ground, it is seldom as low as that of the air, but most frequently approaches nearly to that of boiling water; and this is the most usual, since when they fall in a certain state of softness they adhere to little pieces of straw and other combustible matter, without inflaming them.

They are not hot enough to burn vegetables when they have fallen.

It has been pretended, in consequence of a certain number of aerolites having been analysed, that their elements are all nearly the same; but have all the parts which compose them been carefully examined? For example, has any notice been taken of

Question whether all these bodies have the same composition?

* Memoir on the stones which fell near L'Aigle, by Fourcroy; Annales du Museum d'Hist. Natur. tom. III. p. 101.

† Memoirs de Lausitz, 1796.

‡ Journal de Physique de Gilbert, tom. XXXI.

that

that brown adherent matter, resembling a varnish, which covered those stones at Benares, as well as those which fell in 1775, and which has again been found on those aerolites, of which a shower took place at Valence in 1806? This gluey matter was also in great quantity in the stones of Stannern; according to the description given of it, it appears to be very like coom, this viscid matter is probably a residue of that, which in the antecedent combustion was not entirely consumed, and to which may be attributed the smoke these stones often emit after their fall.

Some aerolites very different from those commonly seen.

There are also certain aerolites which appear very different from those which have hitherto been analysed; to this class belong those small white stones covered with ice, which fell in Russia*; the white pebbles which, in 1552, made so much ravage near Schleusingen, in Bavaria, and at the gates of Munich†, of which specimens were for a long time preserved. And lastly, the stone that fell in Ireland in 1771,‡ which resembled a grey silicious pebble, like those precipitated at Burgos in 1438,§ which were so light that the largest of them did not weigh quite half a pound, though they were as large as small pillows. This last fact, however singular it may appear, may be considered as undoubted, since it depends on the information given by M. Proust.

They appear at times, and under circumstances having the like relation to globes of fire and earthquakes.

The aerolites, therefore, appear, from all that has been observed, to have so intimate a connection with the globes of fire, that we may be almost assured that in those years in which there have been many firy meteors, there will also be one or more showers of stones. And so likewise as the ignited meteors precede or accompany earthquakes, the aerolites also coincide with those great phenomena. In fact, such years as have been marked by a great number of hurricanes and other similar phenomena, have never failed as to the appearance of showers of stones. As a proof of this, the years 1618, 1650,

* Journal de Physique de Gilbert, tom. XXXI.

† Ibid. tom. XXIX.

‡ Nouveau magasin d'histoire naturelle, par Voigt, tom. L.

§ Journal de Physique, tom. LX.

1654,

1654, 1668, 1674, 1723, 1743, 1753, 1755, 1768, 1812, &c. may be mentioned. Frequently the epocha of an earthquake has exactly coincided with the fall of aerolites, as in 1654, where in the Isle of Funen, in the north of Germany, the same week very violent shocks of an earthquake took place, and a shower of meteorolites fell. The same effects have been felt in Germany and even in Switzerland*. Thus, on the 7th of November, 1742, the day on which the town of Bâle sustained so much damage in consequence of an earthquake, an aerolite fell at Ensisheim, which is a short distance from this town†.

Objections to the theory that these bodies come from the moon.

We have already observed, that the principal hypotheses advanced in order to explain all these phenomena, are reduced to two fundamental positions; one of which may be called *cosmic*, and the other *telluric*. The first has been most generally adopted, especially that which considers aerolites as bodies shot from the moon, which pass beyond the point where the attractions of the earth and of the moon are in equilibrio. It may be said, on this subject, that, in adopting this hypothesis, not the least attention seems to be paid to the difference of weather, nor to the height of the thermometer or barometer, nor to the season or hour of the day in which these aerolites most frequently fall;—notwithstanding there exists, in this respect, very sensible differences, which can scarcely be explained by adopting the theory which makes the aerolites come from the moon. Neither does this theory shew the relation which exists between the fall of these stones and the cloud which always accompanies their fall. This cloud, even in some circumstances, precedes the fall of meteorolites, which proves that it is not formed by the vapours exhaled by the stones, as certain philosophers have pretended. This explanation, besides, would not be admissible, on account of the quantity of such vapours, which ought to be in proportion to those proceeding from the stone itself; and lastly, these bodies thrown

* See the work of M. Bertrand on the earthquake in Switzerland.

† See the analysis of this aerolite, in the Memoir of Fourcroy: Annales du Museum d'histoire naturelle, tom. III. p. 108.

from

from the moon would certainly be consumed to the last atom, in consequence of the distance they have to pass through, and yet the explosion never takes place but near to the earth. The natural philosophers, who adopt this theory, consider these atmospheric stones as lava, which agrees very little with the extremely superficial and slight oxidation of these bodies. The same theory assigns no cause for the explosions which always accompany the fall of aerolites; for, in the eruptions of our volcanos, they are very rarely perceived, or in a manner which is purely accidental. The frequent, and often considerable irregularity of their course, the obliquity of their direction, and its being often nearly parallel to the ground, and still more the undulatory rising and falling, or jumps of one of these stones, which proves a deviation contrary to that occasioned by gravity, are among the many proofs which do not allow of the lunar theory being considered as the most probable.

And, lastly, we may add, that by admitting this theory, we cannot at all explain the slowness of the fall of the aerolites. Bodies falling from the moon would not arrive on the roofs of houses without penetrating them, or damaging them considerably, which circumstance has never been observed to happen. The duration of this phenomenon ought also to be nearly the same, with the slight difference which their volume or their weight might produce; but we know that, in some instances, it has lasted for several minutes; and under some circumstances, undoubtedly very rare, its continuance has been for entire hours.

Besides these difficulties, which are of no small magnitude, there are likewise other phenomena, which it would be difficult to explain by the same theory; and though these phenomena do not absolutely belong to the same species as the meteorolites, yet they are so intimately connected with them, that they can scarcely be separated.

Aerolites and globes of fire greatly resemble each other.

We might, it seems, class with the aerolites those ignited globes which are only distinguished from them because their masses are not metallic—but in other respects, like the atmospheric stones, they fall in the hottest months, and in calm weather.

ther. They burn in the same manner, and describe their course with the same velocity; and at the same time the direction in which they approach the earth is no less variable than that of the aerolites. Their explosions are also nearly alike; and as the meteorolites, especially that of 1772, have been observed to have a rotation round their centre, the same has also been remarked in the globes of fire.

What deserves the greatest attention is, that these globes of fire have, like the aerolites, a rounded form, and are of a gelatinous consistency. In fact, a globe of fire which fell in India in 1218, left, after a tremendous explosion, a large round mass of gelatinous matter of considerable firmness. A grey and spungy mass of the same description was found at Coblentz, after the explosion of a ball of fire*. These remarks are not the only ones that have been made of the same kind. Within a later period, masses of this description have been seen as large as a man's head†. Silbershlag relates, that he had even observed the residue of an ignited globe, which presented a gelatinous appearance, and was of a whitish colour‡.

Shooting stars appear to be the same as the ignited globes.

The ignited meteors, improperly called *shooting stars* do not appear to differ from the globes we are speaking of. And these meteors, like them, leave gelatinous masses, which are erroneously attributed to birds of prey, because they do not contain any thing which indicates an animal origin. But neither the ignited globes nor the shooting stars, constantly leave these residues, and this fact must depend on their being composed of parts entirely combustible, which are consumed by burning before they reach the earth. To this class of phenomena we may ascribe the globe of fire, which, according to Geoffroy, burst in the square of Quesnoy, the 4th of January, 1717, and that which was observed in America in 1800, and in the county of Suffolk in 1802.

* Comment. de rebus in scientiâ naturali et medicinâ gestis, tom. XXVI, part I, p. 179.

† Journal de Physique de Gilbert, tom. VI.

‡ Théorie der 1762, beobachteten Feuerkugel, Leipzick, 1764.

Various showers of fire. They resemble the firy globes. To the globes of fire we may certainly join the showers of fire, from which these last are not distinguishable, except by their great division, whilst in the globes the same substance is concentrated in a single body. A rain of this description produced great ravages in Germany, in the year 823*, and burned entire villages. Another shower of the same nature fell in 1571 in the great Duchy of Hesse, and, after a terrible explosion, ran in the streets, still burning, but without causing any real combustion. A third shower of fire took place in 1678 at Sachsen-Haufen†, and the inflammable matter burned for half an hour in the streets before it was extinguished. Lastly, that which fell on the town of Brunswick (Braunschweig) was so violent, that at the beginning the efforts were ineffectual that were made to extinguish it by water‡.

(*To be concluded.*)

V.

Report upon a Memoir of M. Benard, relating to the physical and chemical Properties of the different rays which compose the solar Light. By Messrs. BERTHOLLET, CHAPTAL, *and* BIOT, *by Commission from the National Institute of France.*

Introduction. M. BERTHOLLET, M. Chaptal, and myself, having been charged to examine a memoir recently presented by M. Berard on the physical and chemical properties of the different rays which compose the solar light, we now proceed to give an account of the same to the class.

Whether caloric and light be the same. The question has long been disputed among philosophers and chemists, whether caloric and light be modifications of one and the same principle, or essentially different from each other. Many systems have been made, in favour of each of these hypotheses;

* Donnernder Wetterhnall. Nurnberg.
† Journal de Gilbert, tom. XXX.
‡ Ibid. tom. XXIX.

potheses; but the only means of deciding must consist in carefully ascertaining by experiment, and precisely fixing the properties essentially attached to caloric and light, connecting them in such points as afford resemblance, and opposing them in such as differ, so as, at length, to ascertain whether the same principle, always constant in its nature, but acting differently upon our senses, and upon different bodies, may be capable of producing all the variety of effects we observe. The attention of several chemists and eminent philosophers has been directed to this research. Thus, Mariotte discovered the obscure caloric radiating in the manner of light, and reflected in the same manner by metallic mirrors; results which have been since confirmed by the experiments of Scheele and Pictet. M. Leslie and Count Rumford have particularly studied the influence which the nature of different substances, and state of their surfaces, enables them to exercise on the radiation of caloric when it enters bodies, or escapes from them. And lastly, M. Prevost, of Geneva, has comprehended all the phenomena of radiating caloric in an ingenious theory, which, if considered only as a systematic disposition (as the author himself does) enables us to collect the phenomena under the same point of view, and connect the same by laws. Very lately M. Delaroche has added to these results a new fact, which seems, in a certain respect, to indicate a gradual and progressive transition between caloric and light. It is, that the rays of obscure caloric pass with difficulty through glass when they issue from a body of a temperature below that of boiling water, but penetrate it more easily, and with a facility always increasing accordingly as the temperature of the radiating body is more elevated, and approaches more nearly to the state in which it becomes luminous. So that if we consider these experiments only, the modification, whatever it may be, which it is necessary to impress on the obscure rays, to put them more and more into a state to pass through the glass, brings them also nearer and nearer to the state in which they must be to penetrate our eyes, and produce the sensation of vision. M. Delaroche has found likewise, that the rays of light, which have already passed through a first

History of former inquiries.

Radiant heat passes through glass better the higher the temperature.

Rays of caloric which have

 plate

been once transmitted, pass again more easily.

plate of glass, are proportionally more capable of penetrating a second; which establishes a new proof of the particular state in which these rays subsist, as well as the modification they acquire. The results here mentioned relate to the motion of caloric; but its chemical action has also been studied in comparison with that of light. MM. Gay Lussac and Thenard have proved, that all the changes of colour produced by light, may be imitated and effected by obscure heat, and by a temperature not exceeding 100° of the centesimal thermometer.

Obscure heat effects the chemical changes.

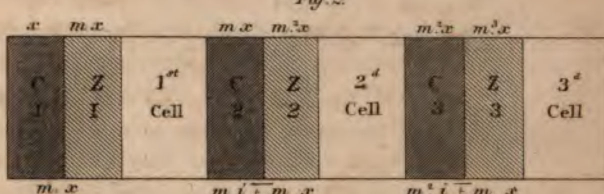
Other phenomena before observed were in proof that, in this comparison of the effects of caloric and light as to raising the temperature of bodies, or to produce chemical changes, a great difference must be admitted between the influences of the rays of different colours. In fact, M. Rochon had announced that the heat produced by the different rays of the same spectrum are not equal. M. Herschel afterwards found that the calorific faculty increases progressively from the violet to the red; and he has even fixed the maximum beyond the red,—so that, according to his experiments, the most calorific rays are entirely, or almost totally, deprived of the faculty of enlightening. MM. Wollaston, Ritter, and Beckmann, having likewise examined the other extremity of the spectrum, namely, that which affords the sensation of violet, have discovered, that this extremity also possesses peculiar properties, and that there exists beyond the violet, rays quite invisible, which, more than all the other rays of the spectrum, possess the faculty of determining chemical combinations. But the experiments of Herschel, though confirmed by several philosophers, have been questioned by others, not less skilful, particularly M. Leslie. It became, therefore, of importance to remove every doubt respecting these uncertainties. It was equally interesting to know whether these invisible rays, or nearly so, which lie beyond the limits of the spectrum, might, nevertheless, possess some of the properties of light: for example, whether reflection from polished glasses could impress upon them that peculiar modification, which M. Malus has denoted by the name of polarisation. M. Berthollet engaged MM. Malus and Berard

Inquiry respecting the experiments of Herschel.

to

Theory of Tides *Fig. 1.*

Fig. 2.



Galvanism

Fig. 3.

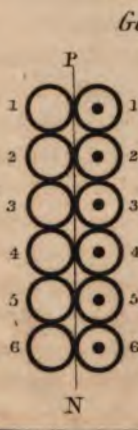


Fig. 4.

Engraved by H.Cooper Chancery Lane.

to undertake this double enquiry. The premature death which has deprived us of our colleague, has also deprived us of the great information which he would, no doubt, have obtained on this subject; as he has already shewn in his excellent discoveries on other parts of optics. But in this respect, at least, the researches he had commenced or projected have not been lost. M. Berard has pursued them with great care; and having terminated them with all possible accuracy, he has presented you with the results.

With regard to apparatus, M. Berard has had great advantage beyond the philosophers who have preceded him in his researches. He made use of the heliostat which Malus had caused to be constructed for the philosophical cabinet of M. Berthollet, and by means of this instrument he obtained a solar ray perfectly fixed, upon which he could operate at pleasure. By decomposing this ray of light with a prism, he obtained a coloured spectrum, which was motionless; and by placing very delicate thermometers in the spaces occupied by the different colours, he was enabled to compare their calorific properties with the greatest certainty. He determined their chemical properties by placing, instead of the thermometers, such chemical compounds as were readily susceptible of alteration.

Heliostat used by M. Berard for the new experiments.

Motionless spectrum.

He first examined the calorific qualities of the different rays; they are known to be unequal. M. Rochon, who first observed this inequality, placed the maximum of heat in the yellow rays, where the property of enlightning is also the strongest. M. Herschel fixed it outside the spectrum, beyond the extreme red rays. The experiments of M. Berard have confirmed those of Herschel relative to the progressive augmentation of the calorific quality from the violet to the red; but he found the maximum of the heat at the extremity of the spectrum itself, and not beyond it. He fixes it at the point where the ball of his thermometer was still entirely covered with the red rays, and he saw that the temperature was gradually less and less, as the ball of the thermometer was removed into the obscurity. Lastly, by placing the ball of the thermometer entirely out of the visible spectrum, at the place where M. Herschel has fixed the

The maximum of radiant heat is at the extreme red, but not beyond it.

At Herschel's extreme point

he found it only one-fifth. Berard's absolute heat was lower.

the maximum of heat, the elevation of temperature above that of the surrounding air, was only one-fifth of what it was in the extreme red rays. The absolute intensity of heat produced was also less in the experiments of Berard, than those of Herschel. Do these differences depend on the material of the prisms and diversity of the apparatuses, or on some other physical circumstance, inherent in the phenomenon itself? On this we cannot decide.

Both pencils issuing from Iceland crystal, had like properties.

M. Berard was desirous of knowing whether these properties, obtained separately in each of the pencils into which a ray of light is divided when it traverses a rhomboid of Iceland crystal. He passed a beam of solar light through a prism formed of a piece of the same kind of crystal. Each of these two spectrums presented the same properties. In both of them the calorific property was gradually less from the violet to the red, and it still subsisted beyond the limits of the visible rays. Consequently, whether this faculty be inherent in the *luminous* rays themselves, or be foreign to them, it is divided along with them when they are separated by the crystal.

The radiant caloric is reflected along with the light, and polarized like it.

But in these operations, the luminous rays are polarized by the crystal. Do the obscure calorific rays undergo a similar effect? In order to determine this, M. Berard received the solar ray on a polished and transparent glass, which polarized part of them by reflection. This reflected ray was afterwards received on a second glass, fixed on an apparatus which allowed it to be turned round the ray with a constant incidence, and lastly, this incidence was itself so determined, that in a certain position of the glass, the reflection ceased to take place. We know, from the experiments of Malus, that a glass can always be so disposed as to answer this condition. This being effected, M. Berard received the calorific and luminous rays after the reflection from a second glass upon a (concave) mirror, and by directing them upon a thermometer, he found that when the luminous reflection took place, the thermometer rose, and consequently the heat was reflected also; but in the case where, in consequence of the position of the second glass, the light passed through without reflection, the heat was transmitted at the

When the light was wholly transmitted, the heat was so likewise.

the same time, and the thermometer did not rise. So that in this experiment, as well as the preceding, the calorific principle, whatever it may be, accompanies the luminous particles, and is never separated from them.

Instead of the beam of solar light employed in this experiment, M. Berard substituted a pencil of radiating caloric emitted from a very hot body, but either scarcely red, or totally obscure. The effect was the same as before. The thermometer rose when the second glass was in a position which would have enabled it to reflect light; but when this reflection was not possible, the temperature continued to be the same as the surrounding air. Hence it is proved, that the invisible particles which compose the rays of obscure caloric are modified by reflection precisely like the particles which compose light.

Caloric radiating from a body not luminous, was polarized.

After having studied the calorific properties of the different rays of the spectrum, M. Berard has attended to their chemical properties. It is known, that when the muriate of silver, or the other white salts of silver, are exposed to light, they become blackened in a very short time. Gum guaiacum, thus exposed to light, passes from yellow to green, as has been observed by Dr. Wollaston; and lastly, Messrs. Gay Lussac and Thenard have shewn us a chemical action still more prompt and energetic; for, by exposing to a ray of solar light a mixture of hydrogen gas and oxigenated muriatic acid in equal volumes, a detonation instantly follows, of which the product is water and muriatic acid combined together. These several phenomena have served M. Berard as reagents to examine and put in evidence the chemical powers of the different rays of the spectrum; for, by putting into the spaces occupied by the different colours small pieces of card impregnated with muriate of silver, or small phials filled with a mixture of the two gases, he was enabled to judge of the energy of the cause by the intensity and rapidity of the chemical changes produced in the substances so exposed to the different rays. In this manner he ascertained, that the chemical properties are, in fact, the most intense towards the violet extremity of the spectrum, and that they extend, as

Chemical properties of rays at the violet extremity of the spectrum.

M. Ritter

M. Ritter and Dr. Wollaston had announced, a small distance beyond that extremity. And farther, by leaving the substances exposed for a certain time to the action of each ray, which the immobility of his spectrum enabled him to do, he succeeded in observing sensible effects, though of an intensity continually decreasing, in the indigo and blue rays, whence we may consider it as extremely probable, that if he could have employed re-agents still more sensible, he might have seen effects of the same description, though more feeble even in the other rays. To shew clearly the extreme disproportion which exists in this respect in the energies of the different rays, M. Berard concentrated by a lens, all that part of the spectrum which extends from the green to the extreme violet, and he collected in the same manner by another lens, all that portion which extends from the green to beyond the extremity of the red. This last beam was united in a *white spot*, of which the eye could scarcely support the brilliancy; but nevertheless the muriate of silver remained exposed for more than two hours, to this strong light, without any sensible alteration. On the contrary, by exposing it to the other beam, of which the light was much less vivid, and the heat less strong, it became blackened in less than ten minutes. M. Berard concludes from this experiment, that the chemical effects produced by light are not owing to the heat alone, which it developes in bodies by combining with their substance; because on this supposition the faculty of producing chemical combinations ought, it should seem, to be stronger in the rays which have the greatest power of producing heat. But we may, perhaps, find less difficulty in these two manners of contemplating the subject, if we pay attention to the diversity of results, which may be produced by the same agent placed in different circumstances, and also that agents of a nature totally dissimilar, may nevertheless determine combinations perfectly identical when they are used.

These are not governed either by the light or the heat.

probation the re- rters.

Such is the abstract of the principal facts which M. Berard has established in his memoir. To great accuracy he has joined a most methodical exposition. He has presented the physical properties of the different rays, as the results of an experimental

experimental research, of which he forbears to seek the cause by hypothesis; and he has constantly restricted himself to terms sufficiently general to allow of their being applied equally, whether the properties described may belong to principles of a nature really different and combined with light, or whether they may simply arise from original differences between the particles of one identical principle, which, according to the various circumstances of mass or velocity, or both united, might become capable of producing chemical combination, vision, or heat.

Without undertaking to decide between two opinions, which both of them go beyond the conclusions afforded by the observed facts, we may nevertheless consider their respective probabilities and compare the number of hypotheses necessary in each of them to represent the number of facts. If we consider the solar light as composed of three distinct substances, of which the one produces visible light, the other heat, and the third effects chemical combinations, it must be also admitted that each of these substances must be separable by the prism, in an infinity of different modifications like light itself, because we find by the experiments, that each of these three principles, whether chemical, illuminating, or calorific, is diffused, though not in like proportions, over the whole of the spectrum. So that we must conceive in this hypothesis, that there exists as it were three spectrums superposed one upon the other; namely, a calorific spectrum, a chemical spectrum, and a luminous spectrum. We must likewise admit that each of the substances which compose the three spectrums, and even each of the particles of unequal refrangibility, which compose these substances, are endued, like the particles of visible light, with the property of being polarized by reflection, of afterwards eluding the reflecting energy in the same positions as the luminous particles, &c. Now instead of this complication of ideas, let us simply conceive, agreeably to the phenomena, that light is composed of a collection of rays unequally refrangible, and consequently unequally attracted by bodies; which supposes original differences in their masses, their velocities, or their affinities. Why should these rays, which

They discuss the question whether there be three kinds of rays, or one kind having three distinct properties.

Reasons why we should

admit only one kind of rays.

which differ in so many other respects, produce upon thermometers and upon our organs, the same sensations of heat and of light? Why should they possess the same energy to form or to disunite combinations? Would it not be more consistent with natural effects, that vision should not be operated in our eyes, except under certain limits of refrangibility, and that an extreme of this quality, either too much or too little, should render the rays alike unfit to produce the effect. These rays may, perhaps, be visible to other eyes than ours; and may probably be so even to certain animals, and then the marvellous in their vision would disappear, or rather it would indicate a fact in the general mode of action of light. In a word, we may conceive the calorific quality and that of chemical power, to vary through the whole extent of the spectrum at the same time as the refrangibility, but according to different functions; so that the calorific quality shall be at its minimum at the *violet extremity* of the spectrum, and at its maximum at the red extremity; while on the contrary, the chemical faculty being expressed by another function, would be at its minimum at the red extremity, and acquire its maximum at the violet extremity or a little beyond it. This single supposition, which is only the expression of the most simple of the phenomena, equally satisfies all facts antecedently observed, and it moreover gives a reason for those which M. Berard has established, and even anticipates them. In fact, if all the rays which produce these three classes of phenomena be light, it must follow that these phenomena will be subject to the law of polarity in its passing through Iceland crystal, or in being reflected from a polished glass at a determinate incidence; and when the rays shall have received these modifications, it must follow, that they will be reflected from another glass, if duly placed for the exercise of its reflecting force upon light; and on the contrary, when this force is nothing with regard to the visible luminous particles, the invisible light will not be reflected; for the same cause, which occasions reflection to take place or not, appears to be exerted on all the rays whatever be their refrangibility, and consequently it must act upon the invisible rays; the condition of visibility or invisibility being relative to our eyes, and

N. B.

not

not universally to the nature of the particles themselves, which produce our sensations. But though this manner of regarding the facts appears to us the most natural and simple, we cannot but approve the wise reserve of M. Berard, by which he has governed his writing, and has avoided any decided conclusions upon questions, respecting which experiment has not yet afforded any determinate results.

Recommendation to the class.

The class has attended with pleasure to the detail of these interesting experiments, when they were presented by the author, on that day in which he shared with M. Delaroche the prize, proposed for the specific heats of the gases. We submit to the class, that they should confirm by their approbation this new and valuable work, and we consider the same as very worthy of being printed in the collection of Memoires des Savans étrangers.

(Signed) BERTHOLLET,
CHAPTAL AND BIOT, Commissaries.

VI.

On the Theories of the Excitement of Galvanic Electricity; by WILLIAM HENRY, *M.D. F.R.S. &c**.

Experiment of Bennet: that metals are electrified by contact.

SEVERAL theories have been framed to account for the origin of the electricity, which is excited by the Galvanic pile, and by similar arrangements. Of these, the first in the order of time was proposed by the distinguished philosopher† to whom we are indebted for some of the earliest, and therefore the most difficult, steps in this department of science. The hypothesis was suggested by a fact, which may be considered, indeed, as fundamental to it. It had been observed by Mr. Bennet, so long ago as the year 1788, and afterwards confirmed by Volta himself, that electricity is excited by the simple appo-

* Manchester Memoirs, II. N. Ser. 293.

† Signor Volta, in Nicholson's Journal, 8vo. i. 135.

sition

sition of different kinds of metals. The best way of exhibiting this fact is to take two discs or plates, the one of copper, the other of zinc; to apply them to each other, for an instant, by their flat faces, and afterwards, separating them dexterously, to bring them into contact with the electrometer. The instrument indicates, by the divergence of its gold leaves, what kind of electricity each of the plates has acquired; which proves to be positive in the zinc plate, and negative in the copper one.

Volta supposed the electricity to be set in motion by the metals.

To explain the phenomena, in the experiment which has been just described, it has been supposed by Volta, that, during the contact of the plates, a movement of the electric fluid takes place from one plate to the other; and that the zinc acquires just as much as the copper has lost. The metals, therefore, he denominates *motors of electricity*, and the process itself *electromotion*, the latter of which terms has been adopted by Mr. Davy. From subsequent experiments, Volta ascertained that the metals stand to each other, in this respect, in the following order; it being understood that the first gives up electricity to the second; the second to the third; the third to the fourth; and so on:

Silver,
Copper,
Iron,
Tin,
Lead,
Zinc.

His theory that the plates of the pile are electrometers, and the fluids mere conductors.

It is to this transference of electricity, that Volta ascribes the whole of the phenomena, exhibited by Galvanic combinations. According to his view, the interposed fluids act entirely by their power of conducting electricity, and not at all by any chemical property. The effect of a series of Galvanic plates, or of a Galvanic pile, he believes to be nothing more than the sum total of the effects of several similar couples or pairs. Why the evolved electricity is determined to one end of the series, and exists there in its greatest force, I shall attempt to explain by the following illustrations.

Illustration.

If a plate of zinc be brought into contact, on both sides, with a plate a plate of copper, it may be considered as acted upon, in opposite directions, by equal forces, which destroy each other. No alteration, therefore, takes place in its state of electricity; nor does any change happen, even when we substitute, for one of the copper plates, a third metal; on account of the trifling difference between the electromotive powers of bodies of this class. But liquids, possessing this power in only a very small degree, may be brought into contact with one of the zinc surfaces, without impairing the electromotive effect; and acting merely as conductors, they convey the excited electricity from the zinc plate, across the contiguous cell, to the next copper plate.

Alternate plates of zinc and copper would produce no more electro-motion than a single pair: but if a plate of fluid were interposed between each pair, the electro-motive power of this last would be too small to produce electro-motion at its surfaces, and each pair would have its full energy.

Let us imagine, then, a series of copper and zinc plates, arranged in pairs for any number of repetitions; (See the Diagram in plate 5, fig. 2,) with cells between each pair for the purpose of containing a fluid. Before these cells are filled, every copper plate will, according to the hypothesis, be in the state of negative, and every zinc plate in that of positive electricity. Let us farther suppose the natural quantity of electricity in each copper and zinc plate, before they are brought into apposition, to be denoted by q, and that, when the electricity has passed from the copper to the zinc, the ratio of the quantities in each may be as $1 : m$*. Let now the cells be filled with a conducting fluid; every pair of contiguous plates of copper and zinc will still maintain their relative proportions of electricity, viz. as $1 : m$. But, by reason of the conducting power of the fluid, the electricities of the first zinc and second copper plates will be equalized; as, in succession, will be also those of the zinc plate 2, and copper plate 3, &c. Now in order to find the relative quantities of electricity in the several pairs of plates, when an equilibrium in the arrangement is effected, if n equal the number of pairs of plates, then $2\,n\,q =$ the total quantity of electricity in all of them taken together. Let $x =$ the quantity of electricity in the first copper plate of the series;

Numerical statement by symbols with reference to a figure.

* For the algebraical expression of this theory, which, in the paper as originally read, I had stated in common numbers, I am indebted to my friend Mr. Dalton.

then

then by hypothesis, $m\,x$ = that of the contiguous or first zinc plate; also $m\,x$ = the quantity in the second copper plate (by reason of the conducting fluid;) but $1 : m :: mx : m^2x$ = the quantity in the second zinc plate. In like manner the quantities in the successive copper and zinc plates may be found, and will constitute this series;

$$\begin{array}{l} \quad\quad\quad\quad\quad\;\; 1 \quad\; 2 \quad\;\; 3 \quad\;\; 4 \quad\quad\quad\quad\; n \\ \text{Copper plates, } x,\ mx,\ m^2x,\ m^3x,\ \&c......mx^{n-1} \\ \text{Zinc plates, } mx,\ m^2x,\ m^3x,\ m^4x,\ \&c......mx^{n} \end{array}$$

The plates are electrified in each successive pair, in geometrical progression;

Hence it appears that the quantities of electricity in the successive plates of copper or of zinc form a *geometrical* progression, the ratio of which is m. Also the total quantities of electricity in the successive pairs of plates form a series in *geometrical* progression, as under.

Pairs of pl.	1	2	3	4	
Quant. of El.	$\overline{1+m}\cdot x$	$m\cdot\overline{1+m}\cdot x$	$m^2\cdot\overline{1+m}\cdot x$	$m^3\cdot\overline{1+m}\cdot x$	&c.

From the above theory of Galvanic action it necessarily follows, that if the effect of a pile be in proportion to the *difference* in the electricities of the first and last plates of the series, a pile of 50 pairs will not be exactly half so energetic as one of 100 pairs, but somewhat less; because the differences in the terms of a geometrical series increase as the terms increase. But, in the present instance, there is great reason to apprehend that the ratio of 1 to m is very nearly that of equality. If so, the geometrical series for a moderate number of terms, will scarcely differ from an arithmetical one. This accords very nearly with experience; for it has been determined by Volta, that if a combination of 20 pairs of plates produce a given effect on the electrometer, a series of 40 will produce double the effect; one of 60 triple, and so on. At the same time it is probable that the electric intensity of the plates, composing each pair, relatively to one another, continues unaltered, notwithstanding the change in their absolute quantities of electricity.

but scarcely differing from an arithmetical one.

Explanation of the neutral middle pair.

When a connection is established between the two extremities of a series like the above, for example between the third zinc plate, or its contiguous cell, and the first copper plate, the

opposite

opposite electricities tend to an equilibrium. The third pair loses a share of its electricity, which is gained by the first; and the intermediate pair, being placed between opposite forces of perhaps equal amount, remains *in equilibrio*. Hence, in every galvanic arrangement, there must be a pair of plates, at or near the centre in the natural state of electricity. A communication between the two extremities of a pile would therefore reduce it to a state of permanent inaction, if there did not still exist some cause, capable of disturbing the equilibrium. On the hypothesis of Volta, this can be nothing else than the property of electro motion in the metallic plates, which has been described as the primary cause of all the phenomena.

A pile communicating in a circle, acts by its electromotion.

This theory, on first view, appears sufficiently to explain the facts on electrical principles, without the interference of chemical action. Consistently with the hypothesis, different fluids, when made parts of Voltaic arrangements, produce effects more or less energetic, as they are more or less active in conducting electricity; the only property, according to Volta, that can be considered as influencing their efficiency in the pile. There are several facts, however, which, if not absolutely irreconcilable with the hypothesis, are certainly not at all explained by it. Why, for instance, it may be asked, when pure water forms a part of the arrangement, is the action of the pile suspended by placing it in an exhausted receiver, or in any of those gases that are incapable of supporting oxidation? Why is its efficiency increased by an atmosphere of oxigen gas, or by adding to the water in the cells several fluids in a proportion not sufficient to change materially its conducting power? Why is the nitric acid, though a worse conductor of electricity, than the sulphuric, more active in promoting the energy of the apparatus? Why is the power of these combinations proportional to the disposition of one of the metals composing them to be oxidized by the interposed fluid? These facts undoubtedly suggest that, in some way or other, the chemical agency of the fluids employed is essential to the sustained activity of the pile. The principle has even been conceded by some

In this theory the fluids differ only by their conducting powers.

But this does not agree with the facts.

The fluids operate by chemical agency.

some distinguished electricians, who have attempted to explain it in different ways.

Cuthbertson's theory, that the electromotion is assisted by the change of oxidation.

To account for the effect of the interposed fluids, Mr. Cuthbertson has suggested a theory which is both ingenious and sufficiently feasible*. With Volta he assumes the electromotive change in the metals to be the first in the order of phenomena. And when (he observes) the copper has given, and the zinc has received, all the electricity, which their mutual powers require, if any menstruum be presented, which is capable of effecting a change in the metallic property of the two bodies, a change in their electrical states must, at the same time, happen. But as the alteration of metallic property is only superficial, the change of electrical condition will also be only at the surface; and the interior part of the zinc plate, retaining its property of resistance, the electric fluid, evolved at its surface, will necessarily be propelled forwards, *through* the menstruum, to the next copper plate of the series. This, however, can only happen in a progressive manner, because the fluid is but an imperfect conductor, a condition indispensable to the maintenance of any galvanic intensity.

Difficulty.

The explanation of Mr. Cuthbertson is, unquestionably, a valuable supplement to the theory of Volta, inasmuch as it takes into account the efficiency of chemical menstrua. These, consistently with his view, will evolve electricity the more freely, in proportion as they destroy more rapidly the metallic property of the plates of zinc. The hypothesis, however, is defective, because it fails to account for some of the phenomena;—why, for example, the action of the menstruum is chiefly, if not entirely, exerted in oxidizing and dissolving the zinc plates; and why the evolution of hydrogen gas, or of nitrous gas, occurs chiefly at the copper surfaces.

Fabroni's hypothesis, that oxidation *causes* the electricity.

An hypothesis, originally suggested by Fabroni, and reversing those which have been already stated, has been adopted by several eminent philosophers in our own country. It assumes the oxidation of the metals composing galvanic arrangements

* Nicholson's Journal, 8vo. ii. 287.

to

to be the *cause*, and not the *effect*, of the evolution of electricity. In the solution of a metal (it has been observed by Dr. Wollaston*,) it would appear, that electricity is evolved by the action of the acid upon the metal; and, in cases where hydrogen is disengaged, that this evolution is required to convert the hydrogen into gas. When a piece of zinc and another of silver, are immersed in very dilute sulphuric acid, the zinc is dissolved, and yields hydrogen gas; the silver, having no power of decomposing water, is not acted upon. But as soon as the two metals, placed under the diluted acid, are made to touch, hydrogen gas arises also from the surface of the silver. In this case, it is added, we have no reason to suppose that the contact of the silver imparts any new power; but merely that it serves as a conductor of electricity, and thereby occasions the formation of hydrogen gas.

Dr. Bostock's theory, viz.

The chemical theory of the galvanic pile, though already suggested in general terms, may be considered, however, as having been a mere outline, till Dr. Bostock undertook to give it greater distinctness and consistency*. To the extended hypothesis which he has proposed, it is necessary to admit, as a ground-work, the three following postulates: 1stly, that the electric fluid is always liberated or generated, when a metal or other oxidizable substance unites with oxigen; 2dly, that the electric fluid has a strong attraction for hydrogen; and 3dly, that when the electric fluid, in passing along a chain of conductors, leaves an oxidizable substance, to be conveyed through water, it combines with hydrogen, from which it is again disengaged when it returns to the oxidizable conductor.

oxidation liberates electricity, which becomes united with hydrogen, and is conveyed by it to the next ox. conductor.

Explanation.

To the efficiency of the pile, two circumstances, it is observed by Dr. Bostock, are essential; that the electric fluid be disengaged; and that it be confined and carried forward in one direction, so as to be concentrated at the end of the apparatus. The first object is fulfilled by the oxidizement of the zinc; the second, Dr. Bostock supposes, is effected by the union

* Phil. Trans.

* Nicholson's Journal, 8vo. iii. 9.

union of the evolved electricity with nascent hydrogen, and by the attraction of the next copper plate for electricity. At the surface of this plate, the hydrogen and electricity are supposed to separate; the hydrogen to be disengaged in the state of gas, and the electricity to be conveyed onwards to the next zinc plate. Here, being, in some degree, accumulated, it is extricated in larger quantity, and in a more concentrated form, than before. By a repetition of the same train of operations, the electric fluid continues to accumulate in each successive pair; until, by a sufficient extension of the arrangement, it may be made to exist at the zinc end of the pile in any assignable degree of force.

Comparison of this with Cuthbertson's theory.

The hypothesis of Dr. Bostock agrees, then, with that advanced by Mr. Cuthbertson, in pointing out the more oxidable metal as the source of the electricity, which is put in action by galvanic arrangements. It goes farther, however, and defines that change, which Mr. Cuthbertson was satisfied with terming, in general language, "a loss of metallic property," to be the process of oxidation; and it adds, also, the important and necessary explanation of the transmission of hydrogen across the fluid of the cells, and the appearance of hydrogen gas at the surface of the copper plates. In these respects, it is certainly more adequate to account for the phenomena. It is chiefly objectionable, inasmuch as the data on which it is founded are altogether gratuitous. For what other evidence have we, than those very phenomena of the pile, which the theory is brought to explain, that electricity *is* evolved by the oxidation of metals, or that hydrogen *is* capable of forming, with the electric fluid, a combination so little energetic as to be destroyed by the mere approach of a conducting body? The theory is imperfect, also, in taking no account of that change in the relative quantity of electricity in two metallic plates, which, according to the observations of Bennett and Volta, must necessarily happen when their surfaces are put in apposition*.

Objections.

* Berzelius seems to have proved, by a most acute and ingenious experiment, in his "Electro-chemical Principles," published in our

The

The discoveries of Mr. Davy, respecting the chemical agencies of the electric fluid, have led him to a theory of the Galvanic pile, intended to reconcile, in some degree, the hypothesis of Volta with that of the philosophers of our own country. It is admitted, by this acute reasoner, that the action of the menstruum, contained in the cells, is absolutely essential to the activity of Galvanic arrangements; and that the two circumstances even bear a proportion to each other. Notwithstanding this concession, he is disposed to consider the movement of electricity which takes place on the contact of two metals, as the cause originally disturbing the equilibrium; and the chemical changes as secondary, and chiefly as efficient in restoring the balance.

Davy's theory that the chemical action gives energy: but the el. of contact is the first mover.

For example, in a pile of copper, zinc and solution of muriate of soda, in its condition of electrical activity, the communicating plates of copper and zinc are in opposite electrical states. And solution of muriate of soda being composed of two series of elements, possessing contrary electrical energies, the negative oxygen and acid are attracted by the zinc, and the positive hydrogen and alkali by the copper. An equilibrium is thus produced, but only for an instant; for muriate of zinc is formed and hydrogen is disengaged. The positive energy of the zinc plates, and the negative energy of the copper ones, are consequently again exerted; and thus the process of electromotion continues, as long as the chemical changes are capable of being carried on.

Example. The electrical energies of the principles of the fluid, and the play of affinities are admitted into consideration.

The most obvious objection, which presents itself against the theory of Mr. Davy, is, that if the chemical agents forming part of a Galvanic arrangement, be merely effectual in restoring the electric equilibrium, no adequate source is assigned of that electricity which gives energy to the apparatus. In other words we perceive, in such a process, nothing more than a constant disturbance of the balance of electricity by the action of the plates, and an immediate renewal of it by the agency of the chemical fluids. According to the hypothesis, the produc-

Objections.

Journal, XXXIV, 161, that oxidation of the zinc in the pile is not the cause of the electricity and its direction.

tion and annihilation of Galvanic energy are carried on in a circle, leaving unexplained that immense evolution of electricity, which is manifested by the most striking effects, both in occasioning the combustion of bodies, and in disuniting the most refractory compounds.

Remarks. The electromotion by the plates and the chemical agency of the fluids are both operative.

On the whole, the electromotive power of the plates, and the chemical agency of the interposed fluids, appear to be the only circumstances, that can be brought to explain the efficiency of the Galvanic pile. To decide which is to be considered as the cause, and which as the effect, is a difficulty not peculiar to this case, but common to every other, where two events, that are invariably connected, are not distinguished by an appreciable interval of time. The most defensible view of the subject, however, seems to me to be that which attributes the primary excitement of electricity to the chemical changes. But it may be questioned whether the whole of the effect arises from the oxidizement of the more oxidable metal; and whether it is not essential to the activity of the pile that one at least of the elements of the interposed fluids should be incapable of entering into union with the negative metal. For example, in a pile composed of zinc, copper, and solution of muriate of soda, the oxygen of the water and the muriatic acid, both of which are negative as to their electrical state, are attracted by the zinc, and have their electricities destroyed. But the hydrogen and alkali, having do affinity for copper, except what arises from a difference of electrical habitude, deposit upon that metal a part of their electricity. The electromotive power of these plates now becomes efficient, and determines the current to one end of the apparatus, in the manner already described in a former part of this essay.

The decomposition of interposed and partial conductors.

Another series of Galvanic phenomena, the explanation of which is attended with some difficulty, are the decompositions that take place in imperfect conductors, forming an interrupted circuit between the two extremities of the arrangement. When two wires, for example, which are inserted into the opposite ends of a tube containing distilled water, are connected with the extremities of the pile, the positive wire, if of an oxidable metal,

metal, becomes oxidized, but if of a non-oxidable metal oxygen gas is evolved from it, whilst, in both cases, a stream of hydrogen gas proceeds from the negative wire. Why, it may be asked, do the elements of water, thus disunited, arrange themselves at a distance from each other? If the particle of water, which has been decomposed, be imagined to have been in contact with the extremity of the positive wire, the hydrogen must have been transmitted in an invisible state to the negative wire: but if the decomposed water were in contact with the negative pole, then the oxygen must have passed imperceptibly to the positive wire.

Why are the parts separated at a distance.

These appearances have been explained by Dr. Bostock on the same hypothesis, by which he has accounted for the phenomena of the pile. The electric fluid, he imagines, enters the water by the positive wire, and is there instrumental either in oxidizing the metal or in forming oxygen gas. In either case, the decomposition of the water must furnish hydrogen, which, uniting with the electric fluid, is carried invisibly to the negative pole, the attraction of which for electricity again occasions the separation of hydrogen, and its appearance in a gaseous state. This theory, however, is liable to some objections.

Invisible transference of chemical elements.

1. It explains the decomposition of those bodies only, which contain hydrogen as one of their elements. And though it has been ably contended by Mr. Sylvester, that the presence of water is, in every case, essential to Galvanic decompositions, yet the fact does not appear to be sufficiently established. Even if it were verified, the agency of moisture might be supposed to consist in its giving that peculiar interrupted transmission, on which the efficacy of Galvanic electricity in disuniting the elements of bodies seems much to depend.

Remarks on this theory.

2. If the postulate of Dr. Bostock be granted, that electricity is evolved by oxidation, we shall be entitled to assume the reverse as equally true, viz. that electricity is absorbed when oxygen passes to the state of gas. In cases, where the positive wire is of an oxidable metal, the phenomena accord sufficiently with the theory; for by its oxidation, electricity may be supposed to be liberated, and to form the required combination with hydrogen.

hydrogen. But when the positive wire is of a non-oxidable metal, oxygen gas is disengaged ; and in the production of this gas the electric fluid might be expected to act, instead of being employed in carrying hydrogen to the negative wire.

Davy's theory of decomposition by the pile. Original and inherent disposition of bodies to certain electric plates cause them to pass to the opposite poles of the pile, &c.

The same class of phenomena has been explained by Mr. Davy on a different theory. According to his view, bodies which are capable of entering into chemical union, are invariably in opposite electrical states, oxygen for example, is negative and hydrogen positive. From the known laws of electrical attraction and repulsion, it will follow that oxygen will be attracted by positive and repelled by negative surfaces, and the contrary process will happen with respect to hydrogen. It is easy then to conceive that these opposite attractions may produce the decomposition of water. To explain the locomotion of its elements, we may imagine a chain of particles of water, extending from the point P to the point N, *fig.* 2, and consisting each of an atom of oxygen united to an atom of hydrogen. In fig. 2, the combination is represented as undisturbed, and the chain is consisting of six atoms of water. But when the attractive force of the point P for oxygen, and N for hydrogen, begin to act, an atom of oxygen and another of hydrogen are removed, as shewn by fig. 3, and new combinations happen between the remaining atoms ; the second of oxygen uniting with the first of hydrogen, and so on. The terminating atoms being supposed to be removed, a new change will follow similar to the first, and thus the process will continue to be carried on, not only when the chain of particles is a short one, but when it extends to a very considerable length.

This doctrine is founded on the generalization of observed facts.

The theory of Mr. Davy, which I have thus attempted to illustrate, derives probability from its being founded on a general property of bodies (their different electrical energies) which appears to be established experimentally, as far at least as experiment can be applied to so delicate a subject. It has the advantage also of explaining a number of facts, chiefly arising out of his own researches, which scarcely admit of being brought under any former generalization. Thus the invisible transference of an element to a considerable distance, even through fluids

fluids having a strong affinity for it, (of sulphuric acid for example through liquid ammonia) which is inexplicable on any antecedent theory, is sufficiently explained by this. The ingenious speculation of Dr. Bostock limited the carrying power of electricity to its action on hydrogen, a defect not imputable to him, but to the state of the science at the time when he wrote. Since that period, the discoveries of Mr. Davy have been unfolded by a train of experiment and induction which is probably not surpassed by any thing in the history of the physical sciences, and which will form a durable monument of the genius and industry of their author.

VII.

Inquiries concerning the mutual Decomposition of soluble and insoluble Salts. By M. DULONG*.

THE phenomena of the mutual decomposition of neutral salts, so important in their application to analysis, and for their connexion with the general theory of chemical affinities, have been the object of research by a great number of celebrated chemists, since the time when Bergmann inferred, that the double decompositions which had been observed before his time, are dependent upon the same principle as those which take place when an insulated base or acid are combined; a fact been long before known. The theory which he presents, in his dissertation on the affinities, appears so natural, that not the slightest doubt arises of his accuracy. Consequently all the works which have appeared for the following twenty years on this subject, appear to have been undertaken with a view to extend this theory, rather than to confirm it by new experiments. A great number of anomalies, which were observed by different chemists, were met by more or less complicated expla-

Bergmann's theory of double elective attractions.

* Presented to the National Institute of France, and inserted in the Ann. de Ch. LXXXII, 273.

explanations, frequently very ingenious, but they did not operate to render the truth of a principle suspected, which had been considered as irrevocably established. M. Berthollet, by submitting to new experiments those facts on which the celebrated chemist of Upsal had founded his doctrine, soon discovered that they would admit of another interpretation, and his learned researches have led him to an explanation of the phenomenon of the mutual decomposition of the neutral salts, which is no less simple than the first, but has this immense advantage beyond it, of predicting, without exception, all the phenomena from a mere knowledge of one of the most interesting properties of these bodies.

Discoveries and doctrine of Berthollet.

The insoluble salts have not been duly attended to.

Chemistry, therefore, leaves nothing further to be desired with regard to the decomposition of the salts to which the principle of M. Berthollet is applicable; namely, relative to the soluble salts. But the *insoluble salts are* also susceptible of exchanging their principles with a great number of the soluble salts. This class of phenomena, though almost as numerous as that which exclusively embraces the soluble salts, and capable of affording new resources to analysis, has not yet been examined in a general manner.

Fourcroy's conclusions in his general work are conjectural.

There is certainly to be found in the system of chemistry of M. Fourcroy, a very extensive table of the mutual decompositions of salts, which comprehends a considerable number of cases of the kind in question. But I am induced to believe, that such decompositions as do not relate to the soluble salts, have not been ascertained by observation, but only anticipated according to the theory adopted by this illustrious chemist. It may be maintained, in support of this assertion, that no memoir has been published upon this subject, and that a great number of facts are contained in these tables, which are not confirmed by observation, and others which are evidently impossible. for example*, we there find that the fluate of barytes is decomposed by the muriate of soda or potash, and that the result is the muriate of barytes, and the fluate of soda or potash. The

Instances.

* Tom. IV, p. 217.

author

author does not point under what circumstances this decomposition can take place; but as, in order to verify it, water must necessarily be used, the fluate of soda, and the muriate of barytes being both nearly equally soluble, they would immediately reproduce the two primitive salts, without leaving any traces of their decomposition. In another place*, the author concludes, that because the phosphoric acid decomposes all the sulphites, it must, by consequence, follow, that all the phosphates can decompose them.

These tables have been composed by mere induction as to the insoluble salts.

I shall confine myself to the mention of these two passages, because it will be sufficient to look through these tables, in order to be soon convinced, that a large part of the facts they contain, have not been ascertained by direct observations; and it must also be remarked, that the theory according to which they have been predicted, is calculated to lead to errors, in this instance, more than in any other.

Chemists have attended to the effect of soluble carbonates on insoluble salts.

The action of the soluble carbonates on the insoluble salts, which appears at first to belong to this class of phenomena, is the only one that has fixed the attention of chemists. It has certainly been long since known, that the carbonates of potash and of soda will decompose a great number of insoluble salts; and this property has frequently been employed to great advanvantage in analysis. But the theory of these decompositions, which, in the opinion of Bergmann, appears so satisfactory, can no longer be supported, since the fundamental principles of its doctrine have been discovered to be incorrect, and the mutual decomposition of salts, in particular, has been proved to belong to a law that is independent of the different degrees of affinity of their principal constituents.

Researches of the author, beginning with this last object.

Having had an opportunity, in some particular researches, to observe a considerably extensive number of facts relating to the mutual decomposition of the soluble and insoluble salts, I endeavoured to determine the general cause of these phenomena, and the method of foreseeing their results without being obliged to retain, by an effort of memory, of which few persons

* Tom. IV, p. 184.

sons would be capable, of all the direct observations which would be requisite to ascertain them. The considerations which have led me to a solution of this problem, being a continuation of the theory of the decompositions of the insoluble salts by the soluble carbonates, I shall begin by submitting to the judgment of the Class, the results of my inquiries upon that subject.

Concerning the action of the soluble Carbonates on the insoluble Salts.

Decomposition of some invaluable salts by carbonates of alkali.

If we examine all the analyses which have been made during the last twenty years, it will be seen that a great number of insoluble salts have been decomposed, either by the carbonate of potash, or by the carbonate of soda. I myself have ascertained by direct experiments, the decomposition of a great number of those which had not been examined with that view; and it may be, therefore, concluded from a well-founded analogy, that all the insoluble salts may be decomposed by the two carbonates I have mentioned. Some chemists have named as exceptions to this rule, the phosphate of lime and the fluate of the same base; but I am convinced that this latter when free from silex, is decomposed like all the others. And so likewise is the phosphate of lime: except that it offers a peculiarity which I shall notice, and which may have misled those who have denied the possibility of its being decomposed. But the soluble carbonates present, in their reaction on the insoluble salts, phenomena which belong to no other kind of salts, and have not yet been observed; these are the phenomena I intend to describe.

Deduction. All insoluble salts are so decomposable.

Carbonate of potash decomposed sulphate of barytes;

but the whole carbonate did not act.

Experiment A. A solution of 15 grammes of carbonate of potash in 500 grammes of water, together with 15 grammes of sulphate of barytes reduced to an impalpable powder, was kept boiling for several hours. It formed a certain quantity of carbonate of barytes; and as the liquor when filtered still produced a strong effervescence with the acids, it was boiled a second time with a fresh quantity of sulphate of barytes; but though the boiling was continued for a length of time, it did not form any more of the carbonate of barytes.

The

The carbonate of soda comported itself precisely in the same manner.

Carbonate of soda had the same effect.

Similar experiments were made with the phosphates of barytes, strontian, the oxalate of lime, &c. a more or less considerable part of the insoluble salt was constantly transformed into a carbonate of the same base; but on reaching a certain limit, the decomposition stopped, although there remained sometimes a very inconsiderable quantity of the soluble carbonate not decomposed.

The carbonates were applied to other insoluble salts with the same consequences.

Since the neutral carbonates when at the temperature of ebullition, retain a much greater quantity of carbonic acid than agrees with the mutual exchange of their principles with the insoluble salts,* it may be supposed that, in the preceding experiments the action of the carbonates was limited from this cause: and in order to determine its influence, these same experiments were repeated; but instead of the saturated carbonates, the fused sub-carbonates were substituted.

The same phenomena occured, that is to say, a portion of the insoluble salt was always decomposed, but the decomposition stopped at a certain period, though the sub-carbonate of potash or of soda, were still in the liquid.

I am convinced by comparative experiments, which it would be useless to relate, that the different degrees of concentration of the solution, produce but very slight variations in the results of this decomposition.

Experiment B. After remaining a long time in a state of ebullition, with a considerable excess of sulphate of barytes, a solution of the sub-carbonate of potash, which consequently would no longer have any action upon this salt, to the filtered liquor was added a small quantity of caustic potash, merely containing the traces of carbonic acid, and it was then again boiled with a new portion of sulphate of barytes. After an hour of ebullition, the residue contained a considerable quantity of carbonate of barytes: as care had been taken to add a great excess of sulphate of barytes, it is evident that the solution

* Mem. d'Arcueil, tom. II. p. 474.

could

Mutual decompositions of the insoluble neutral salts, &c.

could no longer act upon this salt; nevertheless it contained a tolerably large portion of the carbonate of potash not decomposed. Upon fresh quantities of alkali being successively added, and at each addition the above mentioned operation repeated, each time the carbonate of barytes was produced, but by degrees in smaller quantities. After four operations of the same description, there still remained a small portion of carbonic acid in the liquid.

The sub-carbonate of soda, and pure soda presented the ssme phenomena.

As the sub-carbonates of potash and of soda comported themselves in the manner with regard to all the insoluble salts, I considered it to no purpose to subject the others to this proof.

The result of the preceding facts is, that the sub-carbonates of potash and soda can never be entirely decomposed by any insoluble salt. Some comparative experiments made with several of these salts and the sub-carbonate of potash, have proved to me that the relation between the quantity of carbonic acid which is precipitated, and that of the same acid which remains in the liquid when the equilibrium is established, is not the same with regard to the salts formed of the same acid and of different bases, but that it varies from each kind of insoluble salt. It remained to be determined, whether, with respect to the same insoluble salt, this relation would be the same with the sub-carbonate of potash, as with the sub-carbonate of soda, and to ascertain this the following experiment was made:

Experiment C. I took 10 grammes of the sub-carbonate of potash dry, and 7·660 grammes of the sub-carbonate of soda, also dry; quantities which ought each to contain 3·07 grammes of carbonic acid*: these were separately dissolved in 250 grammes

* The proportions of carbonic acid admitted by M. Berard in the sub-carbonate of potash and of soda, appears to be too weak, especially in the latter, 4 grammes of the sub-carbonate of dry soda precipitated by the nitrate of barytes, produced 7·425 grammes of carbonate of barytes. Now admitting with M. Berzelius, that 100 parts of the latter salt contain 21·6 of acid, (and this proportion must be too weak) it is found that in 100 parts of the sub-carbonate of soda, there are 40·09

grammes of water, and each solution was kept in a state of ebullition for two hours on eight grammes of the sulphate of barytes. On analysing the two residues it was discovered that the subcarbonate of potash had produced 2·185 grammes of carbonate of barytes, while the sub-carbonate of soda had only yielded 1·833 grammes. This experiment was twice repeated, and the results did not sensibly differ. Thus two quantities of the sub-carbonate of soda, and of potash which contain the same chemical mass in their base, decompose the quantities of sulphate of barytes which are between them, nearly in the relation of 6 to 5. I shall soon return to the consideration of the consequences that may be deduced from this result.

Mutual decomposition of the insoluble neutral salts, &c.

Since the soluble sub-carbonates can no longer produce the decomposition of an insoluble salt, when by the effect of this decomposition, the acid of this salt is found in a certain relation with the acid or base of the sub-carbonate which is not decomposed, it becomes probable, that by artificially going beyond this limit; inverse phenomena will be produced; which, in fact, the following experiment has fully confirmed.

Experiment. D. Seven grammes of chrystallised neutral sulphate of potash, and six grammes of the dry sub-carbonate of potash, were dissolved in 250 grammes of water. This mixed solution was boiled with the sulphate of barytes, and after several hours of ebullition this latter salt gave not the slightest indication of decomposition. The filtered liquid placed in the same circumstances with the carbonate of barytes produced a considerable quantity of the sulphate of barytes; and heated afresh by the carbonate of barytes, no decomposition took place, although it still contained sulphuric acid.

In a similar experiment* made with 15 grammes of the crystallised sulphate of soda and six grammes of the sub-carbonate of soda, the sulphate of barytes was not attached, while the carbonate of barytes was converted into a sulphate, as far

40·09 of carbonic acid and 59·91 of the base. I found by the same means that 100 parts of the dry sub-carbonate of potash contains: of acid 30·70; of potash 69·30.

as

Mutual decomposition of the insoluble neutral salts, &c.

as a certain limit, beyond which there was no longer any action.

Lastly, the sulphate of potash, and the sulphate of soda alone and perfectly neutral, re-acted likewise upon the carbonate of barytes, and produced on one part sulphate of barytes, and on the other, the sub-carbonate of potash or of soda, which remained in solution together with the portion of the sulphate which resisted the decomposition.

A comparative experiment was made on these two salts similar to experiment C.

Experiment E. Twenty grammes of crystallised sulphate of soda, and ten grammes of sulphate of potash, also crystallised, were separately dissolved in 260 grammes of water: and each solution remained in ebullition during two hours, together with 20 grammes of carbonate of barytes. The sulphate soda produced 10·170 grammes of sulphate of barytes, and the sulphate of potash, 9·870. The quantities of the sulphate of soda and of the sulphate of potash used in this experiment were calculated according to the analysis of M. Bérard, and each one ought to contain 4·433 grammes of real sulphuric acid; now the sulphate of soda lost 3·484 grammes which combined with the barytes, and only 10·949 grammes remained in the liquid. The sulphate of potash only precipitated 3·188 grammes, and consequently, ought to yet retain 1·245, whence it follows that the relation between the quantities of sulphurate acid retained by the potash and the soda, under these circumstances, differ very little from the quantities of the sulphate of barytes, decomposed by the sub-carbonates of the same basis. *(Experiment. C.)*

The re-action of the insoluble carbonates and soluble salts, of which the acids can form with the basis of these carbonates insoluble salts, is equally general with that of the soluble carbonates on the insoluble salts. I have verified this fact by direct experiments on a great part of the known salts which unite these conditions, and the following table contains the results.

Mutual decompositions of the insoluble neutral salts, &c.

Carbonate of barytes.	Carbonate of strontian.	Carbonate of lime.	Carbonate of lead.
Sulphate of Potash...	Id.	O.	Id.
Sulphate of soda.....	Id.	O.	Id.
Sulphate of lime.....	Id.	O.	Id.
Sulphate of ammonia.	Id.	Id.	Id.
Sulphate of magnesia.	Id.		Id.
Phosphate of soda.....	Id.	Id.	Id.
Phosphate of ammonia	Id.	Id.	Id.
Sulphite of potash....	Id.	Id.	Id.
Sulphite of soda.....	Id.	Id.	Id.
Sulphite of ammonia..	Id.	Id.	Id.
Phosphite of potash..	Id.	Id.	Id.
Phosphite of soda....	Id.	Id.	Id.
Phosphite of ammonia	Id.	Id.	Id.
Borate of soda......	Id.		Id.
Arseniate of potash...	Id.	Id.	Id.
Arseniate of soda....	Id.	Id.	Id.
Oxalate of potash....	Id.	Id.	Id.
Oxalate of ammonia..	Id.	Id.	Id.
Fluate of soda.......	Id.	Id.	Id.
Chromate of potash...	Id.	Id.	Id.

Note. This table does not comprehend the earthy and metallic salts which have constantly an excess of acid, because their decomposition may be easily foreseen. MM. Thenard and Roard have had an opportunity of particularly observing it in alum, in their interesting work upon mordants. (Annales de Chemies, tome LXXIV, p. 279.)

Before I proceed to the consequences that may be deduced from the observations noted in this table, I shall make some remarks upon several of them.

All those salts which have ammonia as their base, are completely decomposed by the insoluble carbonates found in the same column, the new insoluble salt replaces the carbonate which is decomposed, and the carbonate of ammonia becomes disengaged as it is formed; so that if a sufficient quantity of insoluble carbonate be added, nothing will remain in solution.

When

Mutual decompositions of the insoluble neutral salts, &c.

When the soluble salt has an insoluble base, the decomposition does not meet with any obstacle, but continues until the liquid contains nothing more in solution. The results of the experiment vary slightly according to the nature of the base of the salt made use of. If it be a salt of which the base is lime or magnesia, in proportion as the acid forms with the base of the insoluble carbonate a salt which is precipitated, the lime is precipitated also in a state of carbonate.*

If the base be metallic it almost always forms a salt with excess of oxide, which being insoluble is precipitated.

All these decompositions belong, with regard to theory, to some one of those which are already known; and I have only related them, in order to particularise the characters which are presented by those which are acted upon by the salts, which have for their base potash and soda.

The reaction of the latter on the insoluble carbonates differs essentially from those of the preceding. A small quantity of carbonic acid is always disengaged at the commencement of the operation, but it soon ceases, though the decomposition continues to make progress. No salt of this class is entirely decomposed by any carbonate. The quantity of the insoluble salt which is formed, is more or less considerable, according to the nature of the soluble salt and insoluble carbonate which is used; but when the reaction is no longer possible, there always remains a portion of the acid of the soluble salt in the liquid along with the greater part of the carbonic acid of the decomposed carbonate.

(To be continued.)

* When the sulphate of magnesia is decomposed by an insoluble carbonate, if a sufficient quantity of the latter be not employed, the fluid, after the operation, shews signs of alcalinity, and it may be supposed that by thus separating a portion of the sulphuric acid from the magnesia, the result would be a salt with an excess of the base. But this phenomenon depends upon the sub-carbonate of magnesia being soluble in a solution of the sulphate of the same base. This property furnishes a very convenient method of freeing the sulphate of magnesia from the influence of the iron and manganese which it contains.

A Memoir

VIII.

A Memoir on the Specific Heat of the Gases. By Messrs. F. DELAROCHE *and* BERARD: *To which the prize proposed by the Class of Mathematics and Natural Philosophy of the Institute of France, proposed in* 1811, *has been awarded. Abstracted by the Authors.*

IN the original memoir, of which we here give a detailed extract, we have given an account of the labours of those who have preceded us in the question of the specific heat of the gases. This part of our work not being susceptible of analyses, we must refer to it, and only remark, that, notwithstanding the labours of Crawford, Lavosier, and Laplace, Gay Lussac, and other philosophers, we were not in possession of accurate information on this subject, when our memoir was laid before the Institute*. We shall, therefore, without farther preface, state the results of our experiments, all of which were made in the laboratory of M. Berthollet, at Arcueil, and the processes we have followed in making them.

Accounts of experiments, and history of previous researches.

SECTION I.

Account of the Processes followed in making our Investigation.

1. *Description of the calorimeter.*—The intention of our enquiry was to determine the specific heat of the gases, according to the import commonly attached to these words; that is to say, to determine how much caloric is required to elevate each from a given temperature to another temperature, also assumed and fixed, or, which amounts to the same thing, how much heat

Description of a calorimeter.

* We had deposited in the Office of the Institute, February 3, 1812, a first Memoir containing the principal results of our investigations.

 they

they give out in returning from the higher to the lower of those temperatures. We did not seek to determine the influence which is exerted upon the phenomenon by the change of specific heat, determined in the gases by their dilatation or contraction, which take place by digression or elevation of temperature; a change, which their great dilatability must render more sensible than in other bodies, and of which M. Gay Lussac has proved the existence. This department of enquiry could not have been made with our apparatus but with extreme difficulty.

The means hitherto employed for the determination we sought, having all appeared liable to some objections, we determined to employ another, founded on the following considerations:

Elementary principles.

Let us suppose a constant and uniform source of heat, of which the action shall be totally confined to the body A insulated in the air; then this body will become gradually heated to a point at which, on account of its elevation of temperature, above the surrounding air, it will lose as much as it receives. At this point the temperature will become stationary, if the temperature of the air continues unvaried. On the other hand, it is a principle generally admitted, and of which the truth cannot be disputed, particularly when small differences of temperature are concerned, that the differences of heat lost at each instant by a body insulated in the air, are proportional to the excess of its temperature above that of the surrounding air. Now, since the body A, when once arrived at its maximum, loses as much heat as it receives; and that which it loses in a given time is proportional to the excess of its temperature beyond that of the surrounding air; it must be concluded, that when arrived at this point, the quantity of caloric which is communicated to it by the source of heat, in a given time, is likewise proportional to the same excess.

The instrument was a worm pipe and cooler; to be heated by a current

Let us now make a cylinder of thin copper 15 centimeters (5·89 inches) in height, and 8 (3·14 inches) in diameter, filled with distilled water, and transversed by a worm pipe of more than 1 matre (39·3 inches) in length, forming a spiral of 8 turns,

turns, the two extremities of which terminate without a side the vessel, one above, and the other below; and if a regular current of gas, delivered at an uniform and elevated temperature, be passed through the pipe, this current may be considered as a source of uniform heat, and the cylinder as the body A, consequently, if the same experiment be repeated upon each of the gases, each current will raise the temperature to a fixed point, where it will remain stationary; and, upon arrival at this point, it will follow, from the principles laid down, that the quantity of heat communicated to the cylinder, in a given time, by each current, will be proportional to the excess of that stationary temperature beyond that of the surrounding medium.

of the gas to a maximum.

We shall, therefore, obtain, with great precision, by this means, the comparative specific heat of each of the gases, which can be subjected to this mode of examination. There will be two methods of afterwards comparing with them that of water. The first consists in submitting the cylinder we have here described, and shall distinguish by the name of calorimeter, to the action of a current of water, regularly transmitted with a velocity so low, that it shall scarcely exceed, in its effect, that of the gases; and the second method consists in determining, by calculation, the real quantity of heat which the calorimeter arrived at, its stationary temperature loses in a given time. For we have shewn, that, when arrived at this point, the heat which it loses in a given time, is equal to that which it receives from the current of gas in the same time.

Reference of the different effects to water as the standard.

It may be conceived, that, a long time would have been consumed in raising the temperature of the calorimeter through the whole interval up to the stationary point, by the mere effect of current of heated gas; and that the observation of its heating, during this time, would have been of no value. We, therefore, preferred to elevate its temperature, in the first instance, by a spirit lamp to a term which previous trials had shewn us to be near that at which the temperature would be stationary. It was then left without the lamp, and exposed to the current of heated gas, and its augmentation of temperature observed

Means of precisely ascertaining the maximum.

The vessel was heated till its rate of change was the same as well above as below the maximum.

observed for every ten minutes. But notwithstanding this precaution a very long time would have been required to produce the maximum, and it would have been extremely difficult to ascertain when it had been really attained. We found it more convenient to stop the proceeding as soon as, by the progress of heating, we judged that only two or three tenths of a degree were wanting to arrive at that maximum; and then we elevated the temperature of the calorimeter by the approach of an heated body, by a quantity rather greater than the maximum, at which period the calorimeter being left to itself, lost some of its temperature, notwithstanding the current of gas was continued to be passed through it. We observed also the progress of this refrigeration for every ten minutes, and we stopped the experiment as soon as the retardation of the cooling indicated that the calorimeter was at the same distance from the term, at which its temperature would have been stationary, as it was in the preceding experiment. Taking, therefore, the mean between the final observations, we obtain with accuracy the term at which the temperature of our calorimeter would have been stationary, if the heating action of the gas had been continued for a sufficient length of time.

Temperature, of the calorimeter, how obtained, &c.

The temperature of the calorimeter was determined by means of a thermometer, with a cylindrical reservoir, nearly of the same length as that of the calorimeter itself, in order to shew the mean temperature of all the strata of the water. This instrument was sufficiently sensible to indicate clearly 0·02 of a degree (centigrade.) We shall now proceed to shew, what methods we pursued to produce a constant current of gas, to give it an invariable temperature, to determine the temperature of this current, on its entering and issuing out from the calorimeter, and to appreciate the causes which are independently of this current, might raise the temperature of the calorimeter.

(To be Continued.)

SCIENTIFIC NEWS.

A Treatise on new philosophical instruments for various purposes in the arts and sciences, with experiments on light and colours. By David Brewster, L L. D. F. R. S. E, &c. octavo 427 pages with 12 copper plates. Edinburgh printed 1813, and sold also in London.

The present notice will contain only a concise indication of the subjects contained in this valuable treatise. They all relate to optics and the properties of light, in five books, divided into chapters. The first book affords descriptions of the common micrometer, and seven new micrometers on different principles; viz. the wires, divided object glass, luminous image, mother of pearl, rotatory, eye-piece wire, and their applications. Book II. Instruments for measuring angles when the eye is not at their vertex. Book III. Instruments for measuring distances. Book IV. Instruments for viewing objects under water: another for measuring the refractive powers of fluids, with a method of determining the refractive powers of solids; another instrument for measuring the dispersive and refractive powers of solid and fluid substances, with remarks on the irrationality of the coloured spaces in different mediums; new properties impressed upon light by transmission through diaphanous mediums, and by its reflection from polished surfaces, &c. Book V. New telescopes and microscopes.

Experimental Researches concerning the Philosophy of Permanent Colours, and the best means of producing them by dyeing, calico printing, &c. By Edward Bancroft, M. D. F. R. S. &c. in two volumes octavo, containing 1028 pages. besides

besides an introduction of 33 pages, on the origin and progress of dyeing and calico-printing, a preface of 18 pages, and a full index of 33 pages, with tables of contents to each volume. London 1813.

In the year 1794, Dr. Bancroft, who has been long known and respected by philosophers and manufacturers for his useful researches and discoveries, published his "Experimental Researches concerning the Philosophy of Permanent colours," &c. in one volume, which has since become extremely scarce. The numerous advances made in chemistry since that time have rendered it impracticable to compose a second volume and connect it with the former; for which reason the author has thought fit to compose a new and extended edition of the whole. He has generally used the language of modern chemists; but objects to the new metals of Sir H. Davy, as being improperly classed along with denser bodies already denoted by that term, and while he doubts whether the chlorine of that chemist be a simple or compound body, he thinks Prieur's term of *murigene* would be preferable to chlorine if a new denomination were required. He has divided his work into four parts. Part 1. Treats of the permanent colours of natural bodies, their composition, the various colouring matters, and those colours which being fixable by themselves he calls substantive. Part 2. Relates to colours which require a basis or mordant, and are by him denominated *adjective*. He treats of all the several bodies in the animal and vegetable kingdoms, of this class in this and his third part, and in Part IV he discusses the subject of compound colours.

Mr. W. Henley, Member of the London Philosophical Society, is preparing for the press, a series of chemical tables, which will exhibit the properties of all the present known bodies, the result of their union, the composition of the Oxides, acids and their compounds, with the effects produced by the action of heat, light and electricity; so that the whole shall form

form a complete abstract of the science of chemistry. Upon this I would remark that tabulated results, in every science, form the most useful compendiums as well from the relations of things exhibited by their orderly arrangement, as from the facility with which every required fact can be recurred to. Tables not only shew the fulness of a science but its defects and wants; and from this last advantage the departments open for new researches are clearly seen. From the labours of this author, added to what has been done and is doing by Dr. Thomson, we are intitled to expect great benefits in the acquisition and cultivation of chemical knowledge.

Dr. Smith and Mr. Sowerby have determined to finish their celebrated work "English Botany," by a general index to the 36 volumes, which will be completed on the first of January, 1814. It is intended to arrange the names of the English plants contained in that work, which will amount to nearly 2600, in one part alphabetically, and in another part according to the Linnean system, with such improvements as have been received during its publication. When English botany is completed, Mr. Sowerby hopes to be able to comply with the wishes of his numerous friends, in publishing his "Mineral Conchology" every month. The British and Entic mineralogy will, in all probability, be finished in the course of the next year.

The learned author of the Theory of the Tides, which was concluded in this number, has favoured me with the following note:

"I believe it is not commonly known, that the compass is cursorily mentioned as a familiar illustration by Dante, who wrote about the time at which it is supposed to have been introduced. Parad. Cant. 12.

Si

Si mosse voce, che *l'ago alla stella*
Parer mi fece, in volgermi al suo dove.

A voice arose, harmonious, smooth, and clear,
Which fixed me, like the *needle to the star*,
Turned to the radiant light that poured it on my ear.

July, 1813.

Captain Laskey has at press, a Scientific description of the rarities in that magnificent collection, " The Hunterian Museum," now deposited at the college of Glasgow. It is intended to comprise the rare, curious, and valuable articles in every department of art, science, and literature, which are contained in that great Repository. This interesting work, is expected to appear early in July.

Dr. Marshall Hall, Royal Infirmary, Edinburgh, is preparing a practical work on the physiognomy and attitude of patients, and on the symptoms and diagnosis of diseases.

The whole of the work is intended to bear on diagnosis as its principal and ultimate object.

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END OF THE THIRTY-FIFTH VOLUME.

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